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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From March 24, 1881, *to June* 16, 1881.

VOL. XXXII.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

March 24, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. THE CROONIAN LECTURE.—"Observations on the Locomotor System of Echinodermata." By GEORGE J. ROMANES, M.A., F.R.S., and Professor JAMES C. EWART, M.D. Received March 5, 1881.

(Abstract.)

I.—MORPHOLOGY.

In *Holothuria* the polian vesicle opens freely into a wide circular canal a short distance from the termination of the stone canal. From this circular canal five lozenge-shaped sinuses project forwards, and from each of these two large oval sinuses run forward parallel with each other—the ten oval sinuses becoming continuous with the hollow stems of the tentacles. Injection of the polian vesicle shows that it forms one continuous tube system with the circular canal and its sinuses, oval sinuses and tentacles, ampullæ and pedicels. Unless the pressure is kept up for a considerable time there is no penetration of the injected fluid into the stone canal, and either the ring, the vesicle, or a sinus gives way before the fluid reaches the madreporic plate. Specimens injected with a gelatine mass show that each canal sinus opens into a cæcal tube, which runs forwards internal to the sinuses of the tentacles as far as a wide circum-oral space. This space communicates by well-defined apertures with that portion of the body cavity which lies between the sinuses and the œsophagus, and which is reached through the circular apertures between the sinuses of the circular canal. Each canal sinus has three other apertures in its walls. It opens by a small round aperture into a radial canal, and the two other apertures occur as minute slits, one at each side of the orifice of the radial canal leading into the adjacent tentacle sinuses. When the tentacle into which the sinus opens is protruded, there is no constriction between the sinus and the tentacle; but when the tentacle is retracted, there is a well-marked constriction at the junction of the sinus with the tentacle. The eversion of the perisome and the protrusion of the tentacles are effected chiefly by the shortening of the polian vesicle and the constriction of the longitudinal muscular bands, which run from the inner surface of the body wall between each two adjacent tentacle-sinuses; but the circular fibres of the body wall also assist in the process by contracting immediately behind the group of sinuses, so as to act on them by direct pressure, and also indirectly by forcing the body fluid against them.

The amount of the body cavity fluid is constantly changing. At the entrance to the cloacal chamber there is a circular valve which is constantly dilating and contracting, except when the aboral end of the animal is forcibly retracted. When open, this valve allows water to pass into the respiratory tree; when it begins to retract, water escapes from the cloaca. This alternate opening and closing takes place with perfect rhythm, at a rate of about six revolutions per minute. At the end of every seventh or eighth revolution a large stream of clear water is ejected, which sometimes contains sand and the remains of food particles. When the tentacles are being protruded, more water is taken in at the cloaca than escapes; on the other hand, retraction of the tentacles is preceded by an escape of a large stream of water.

In *Echinus*, two tubes spring from the under surface of the madreporic plate. The one is dilated at its origin, so as to include the greater portion of the plate, and ends in the so-called heart; the other is small, deeply pigmented, and runs along a groove in the heart to open into a circular canal at the base of the lantern. From the under aspect of this circular canal the five radial ambulacral vessels take their origin. Immediately within the oral margin of the shell, and alternating with the inner row of pedicels, are the five pairs of "tree-like organs." If a fine glass cannula be forced through the membrane which extends from the apex of each tooth to the oral margin of the interambulacral plates and sides of the alveoli, coloured fluids may be injected into the space between the membrane and the alveoli of the lantern; the fluid then slowly diffuses upwards into the vesicles around the apices of the teeth. It reaches these vesicles partly by passing directly upwards external to the alveoli; and partly by passing into the cavities of the alveoli and ascending through the circular sinus.

In *Spatangus* the ambulacral circumoral canal has no polian vesicles or sinuses developed in connexon with it. Some of the pedicels have suckers, others are conical and devoid of them, while others again are flattened at their tips, and sometimes split up into segments.

If one of the arms of *Solaster papposa* is divided transversely, and a coloured fluid introduced into the open end of the radial canal, the ampullæ and pedicels of the injected arm are at once distended. The fluid next penetrates the circular canal, polian vesicles, ampullæ, and pedicels of the other arms; but unless considerable pressure be kept up for some time, none of the solution enters the madreporic canal. If, however, the pressure is maintained for several hours with a column of fluid 2 feet high, the fluid ascends through the stone canal and diffuses slowly through the madreporic plate. When a thin slice is then shaved off the plate, the fluid is observed escaping from a small circumscribed area situated between the centre and the margin of the plate, and corresponding in size and position with the termination of the stone canal on the inner surface. The stone canal, gradually increasing in diameter as it passes inwards from the madreporic plate, runs obliquely over its accompanying sinus, till it finally hooks round this sinus to open into the circular canal. Springing from this canal and opposite to each inter-radial space (with the exception of the space occupied by the stone canal) is a polian vesicle. The size and form of these vesicles are largely determined by the amount of fluid in the pedicels. In none of the injected specimens was there any evidence of a communication between the ambulacral vessels and the body cavity, or between the ambulacral and the blood (neural) vessels. There was, however, abundant evidence of communication between the latter and the exterior. When a cannula was introduced into the outer end of the sinus, a coloured solution could be easily forced through the sinus into the circular blood-vessel, and from this into the radial blood-vessels. But when the cannula was introduced into the proximal end of the sinus, the solution rapidly rushed along the sinus and escaped through the madreporic plate, proving that the blood-vessels of *Solaster* communicate far more freely with the exterior than do the water-vessels.

The ambulacral system of the common star-fish only differs from that of the sun-star in having no polian vesicles. *Astropecten*, on the other hand, has polian vesicles; but in it the pedicels have departed from the usual form in being short, conical, and unprovided with terminal suckers. In *Ophiura* the pedicels are morphologically similar to those of *Astropecten*, though shorter and more slender.

They diminish in size as they proceed outwards, and at the ends of the arms are scarcely visible.

Nervous System of Echinus.

The internal nervous system of *Echinus* consists of five radial trunks, which may be traced from the ocular plates along the ambulacral areas external to the radial canals to the oral floor, where they bifurcate and unite with each other so as to form the pentagonal nerve-ring. This ring lies between the œsophagus and the tips of the teeth which project from the lantern. Small branches leave the ring and supply the œsophagus, and lateral branches arise from the several trunks to escape with the pedicels through the apertures of the pore plates. Each trunk lies in a sinus situated between the lining membrane of the shell and the ambulacral radial canal. The lateral branches which accompany the first series of pedicels through the oral floor are large and deeply pigmented. The branches within the auricles are small; those external to the auricles gradually increase in size until the equator is reached, and from the equator to the ocular plates they diminish in size. At the equator, the trunk is wider than at either pole, and it is often partially divided for some distance at each side of the equator by a deep longitudinal fissure.

When the nerve-trunk, after being stained with chloride of gold or with osmic acid, is removed from its sinus, it is seen to be enveloped by a thin fibrous sheath. This sheath contains numerous large pigment cells, and has scattered over it irregular masses of protoplasm which have been deposited from the fluid of the neural sinus.

When the sheath is removed, the trunk is seen to consist of delicate fibres and of fusiform cells. The cells consist of a nucleus and a thin layer of protoplasm, which projects at each end, and terminates in a nerve-fibre.

The lateral branches of the trunk escape along with and are partly distributed to the pedicels; the remainder break up into delicate filaments which radiate from the base of the pedicel under the surface epithelium. When one of the large branches already referred to as escaping with the inner row of pedicels is traced through the oral floor, after sending a branch to the foot, it breaks up into delicate fibres, some of which run towards the bases of the adjacent spines and pedicellariæ, while others run inwards a short distance towards the oral aperture.

Either in connexion with, or anatomically independent of, these filaments from the lateral branches of the nerve-trunks, there arises an external plexus lying almost immediately under the surface epithelium and extending from the shell to the spines and pedicellariæ. The fibres of this plexus closely resemble the fibres of the lateral branches of the trunk, but generally they are smaller in size and have a distinct connexion with nerve-cells. The cells consist of an oval nucleus and of a layer of protoplasm, which is generally seen to project in two, or sometimes in three, directions—the several processes often uniting with similar processes from adjacent cells, so as to form a fibro-cellular chain or network.

In preparations from portions of *Echini* treated with both chloride of gold and osmic acid, we have succeeded in tracing the plexus over the surface of the shell between the spines and pedicellariæ, and from the surface of the shell to the capsular muscles at the bases of the spines; we repeatedly observed delicate fibres passing beyond the muscles to end apparently under the epithelium over the surface of the spines.

In the case of the pedicellariæ the plexus on reaching the stem runs along between the calcareous stem and the surface epithelium, to reach and extend over and between the muscular and connective tissue fibres between the stem and the bases of the mandibles. The plexus, now in the form of exceedingly delicate fibres connecting small bipolar cells, reaches the special muscles of the mandibles. In several preparations delicate fibres appeared to extend towards the sensitive epithelial pad situated on the inner surface of each mandible a short distance from the apex. Although this plexus is especially related to the muscular fibres, lying over and dipping in between them, it is also related to the surface epithelium, and delicate fibres often extend from it to end under or between the epithelial cells.

II.—PHYSIOLOGY.

1. *Natural Movements.*—The ordinary crawling movements of *Astropecten aurantiacus* are peculiar, the ambulacral feet acting the part of walking-poles and cilia combined. Brittle-stars progress by using two opposite arms upon the floor of the tank, with a movement like swimming; at each stroke the animal advances with a sort of leap, and can thus travel at the rate of six feet per minute. The ordinary progression of *Echinus* and *Spatangus* is assisted by the co-ordinated action of the spines, and when placed upon a flat surface out of the water the animal advances by means of its spines alone. In *Echinus* the lantern and pedicellariæ are also used to assist in locomotion.

All the Echinodermata that we have observed are able, when placed upon their dorsal surfaces on the floor of a tank, to recover their normal position on their oral surface. The common star-fish does so by twisting the ends of two or more of its rays round, so as to bring its terminal suckers into action upon the floor of the tank, and then, by a successive and similar action of the suckers further back in the series, the whole ray is progressively twisted round, so that its ambulacral surface is applied flat against the floor. The rays which perform this action twist their semi-spirals in the same direction, and by their concerted action serve to drag the disk and the remaining rays over themselves as a fulcrum. Other species of star-fish, which have not their ambulacral suckers sufficiently developed to act in this way, execute their righting movements by doubling under two or three of their adjacent rays, and turning a somersault over them, as in the previous case. *Echinus* rights itself when placed on its aboral pole, by the successive action of two or three adjacent rows of suckers, so gradually rising from aboral pole to equator, and then as gradually falling from equator to oral pole. *Spatangus* executes a similar manœuvre entirely by the successive pushing and propping action of its longer spines.

2. *Stimulation.*—All the echinoderms that we have observed seek to escape from injury in a direct line from the source of stimulation. If two points of the surface are stimulated, the direction of escape is the diagonal between them. When several points all round the animal are simultaneously stimulated, the direction of advance becomes uncertain, with a marked tendency to rotation upon the vertical axis. If a short interval of time be allowed to elapse between the application of two successive stimuli, the direction of advance will be in a straight line from the stimulus applied latest. If a circular band of injury be quickly made all the way round the equator of *Echinus*, the animal crawls away from the broadest part of the band—*i.e.*, from the greatest amount of injury.

The external nerve plexus supplies innervation to three sets of organs —the pedicels, the spines, and the pedicellariæ; for when any part of the external surface of *Echinus* is touched, all the pedicels, spines, and pedicellariæ within reach of the point that is touched immediately approximate and close in upon the point, so holding fast to whatever body may be used as the instrument of stimulation. In executing this combined movement the pedicellariæ are the most active, the spines somewhat slower, and the pedicels very much slower. If the shape of the stimulating body admits of it, the forceps of the pedicellariæ seize the body and hold it till the spines and pedicels come up to assist.

And here we have proof of the function of the pedicellariæ. In climbing perpendicular or inclined surfaces of rock covered with waving sea-weeds, it must be no small advantage to an echinus to be provided on all sides with a multitude of forceps adapted, as described, to the instantaneous grasping and arresting of a passing frond; for, in this way, not only is an immediate hold obtained, but a moving piece of seaweed is held steady till the pedicels have time to establish a further and more permanent hold upon it with their sucking disks. That this is the chief function of the pedicellariæ is indicated by the facts that, 1st, if a piece of seaweed is drawn over the surface of an *Echinus*, this function may clearly be seen to be performed; 2nd, the wonderfully tenacious grasp of the forceps is timed as to its duration with an apparent reference to the requirements of the pedicels, for after lasting about two minutes (which is about the time required for the suckers to bend over and fix themselves to the object held by the pedicellariæ, if such should be a suitable one), this wonderfully tenacious grasp is spontaneously released; and, 3rd, the most excitable part of the trident pedicellariæ is the inner surface of the mandibles, about a third of the way down their serrated edges—*i.e.*, the part which a moving body cannot touch without being well within the grasp of the forceps. When the forceps are closed, they may generally be made immediately to expand by gently stroking the external surface of their bases.

With regard to stimulation of the spines, if severe irritation be applied to any part of the external or internal surface of an *Echinus*, the spines all over the animal take on an active bristling movement. The tubercles at the bases of the spines are the most irritable points on the external surface.

With regard to stimulation of the pedicels, if an irritant be applied to any part of a row, all the pedicels in that row retract in succession from the seat of stimulation, but the influence does not extend to other rows. A contrary effect is produced by applying an irritant to any part of the external nerve plexus, all the pedicels being then stimulated into increased activity. Of these antagonistic influences, the former, or inhibitory one, is the stronger, for if they are both in operation at the same time the pedicels are retracted.

Star-fish (with the exception of brittle-stars) and *Echini* crawl towards, and remain in, the light; but when their eye-spots are removed they no longer do so. When their eye-spots are left intact they can distinguish light of very feeble intensity.

3. *Section.* — Single rays detached from the organism crawl as fast and in as determinate a direction as do entire animals. They also crawl towards light, away from injuries, up perpendicular surfaces, and, when inverted, right themselves. Dividing the ray-nerve in any part of its length has the effect of destroying all physiological continuity between the pedicels on either side of the division. Severing the nerve at the origin of each ray, or severing the nerve-ring between each ray, has the effect of totally destroying all co-ordination among the rays; therefore, the animal can no longer crawl away from injuries; and, when inverted, it forms no definite plan for righting itself. Each ray acting for itself, without reference to the others, there is, as a result, a promiscuous distribution of spirals and doublings, which, as often as not, are acting in antagonism to one another. This division of the nerves usually induces, for some time after the operation, more or less tetanic-like rigidity of the rays. The operation, however, although so completely destroying physio-

logical continuity in the rows of pedicels and the muscular system of the rays, does not destroy, or perceptibly impair, physiological continuity in the external nerve-plexus; for however much the nerve-ring and nerve-trunks may be injured, stimulation on the dorsal surface of the animal throws all the pedicels and the muscular system of the rays into active movement. This fact proves that the pedicels and the muscles are all held in nervous connexion with one another by the external plexus, without reference to the integrity of the main trunks.

If a cork-borer be rotated against the external surface of an *Echinus* till the calcareous substance of the shell is reached, and therefore a continuous circular section of the overlying tissues effected, the spines and pedicellariæ within the circular area are physiologically separated from those without it, as regards their local reflex irritability. That is to say, if any part of this circular area is stimulated, all the spines and pedicellariæ within that area immediately respond to the stimulation in the ordinary way, while none of the spines or pedicellariæ surrounding the area are affected, and conversely. Therefore we conclude that the function of the spines and pedicellariæ of localising and gathering round a seat of stimulation is exclusively dependent upon the external nervous plexus. If the line of injury is not a closed curve, so as not to produce a physiological island, the stimulating influence will radiate in straight lines from its source, but will not irradiate round the ends of the curve or line of injury.

Although the nervous connexions on which the spines and pedicellariæ depend for their function of localising and closing round a seat of stimulation are thus shown to be completely destroyed by injury of the external plexus, other nervous connexions, upon which another function of the spines depends, are not in the smallest degree impaired by such injury. This other function is that which brings about the general co-ordinated action of all the spines for the purposes of locomotion. That this function is not impaired by injury of the external plexus is proved by severely stimulating an area within a closed line of injury on the surface of the shell; all the spines over the whole surface of the animal then manifest their bristling movements, and by their co-ordinated action convey the animal in a straight line of escape from the source of irritation.

We have, therefore, to distinguish between what may be called the local reflex function of the spines, which they show in common with the pedicellariæ and which is exclusively dependent upon the external plexus, and what we may call the universal reflex function of the spines, which consists in their general co-ordinated action for the purposes of locomotion and which is wholly independent of the external plexus. Evidently, therefore, this more universal function must depend upon some other set of nervous connexions (which, however, we have not been able to detect histologically), and experiment shows that these, if present, are distributed over all the *internal* surface of the shell. For if the internal surface be painted with acid, or scoured out with emery paper and brick-dust, the spines and pedicellariæ, after a short period of increased activity or bristling, become perfectly quiescent, lie flat, and lose both their spontaneity and irritability. After a few hours, however, the spontaneity and irritability of the spines return, though in a feeble degree, and also those of the pedicellariæ in a more marked degree. These effects take place over the whole external surface of the shell, if the whole of the internal surface be painted with acid or scoured with brick-dust; but if any part of the external surface be left unpainted or unscoured, the corresponding part of the external surface remains uninjured. From these experiments we conclude:—1st, that the general co-ordination of the spines is wholly dependent on the integrity of the hypothetical internal plexus; 2nd, that the hypothetical internal plexus is everywhere in intimate connexion with the external, apparently through the calcareous substance of the shell; and 3rd, that complete destruction of the former, while profoundly influencing through shock the functions of the latter, nevertheless does not wholly destroy them.

Echini may be divided into pieces, and the pedicels, spines, and pedicellariæ upon these pieces will continue to exhibit their functions of local reflex irritability, however small the pieces may be. If an entire double row of pedicels be divided out as a segment, and then placed upon its aboral end, it may rear itself up on its oral end by the successive action of its pedicels, and then proceed to crawl about the floor of the tank. We have therefore to meet the question: Is the action of the ambulacral feet in executing these righting movements of a merely serial kind—*a*, *b*, and *c*, first securing their hold on the tank floor, owing to the stimulus supplied by contact, and then by their traction tilting over the globe till *d*, *e*, and *f* are able to touch the floor, and so on; *or* does the righting action depend upon nervous co-ordination? We conclude that both principles are combined, the action of the pedicels being serial, but also assisted by nervous co-ordination. This conclusion is sustained by the experiment of shaving off the spines and pedicels over one-half of one hemisphere—*i.e.*, the half from the equator to the oral pole. When then inverted and forced to use their mutilated pedicel-rows, the *Echini* reared themselves upon their equators, and then, having no more pedicels wherewith to continue the manœuvre, came to rest. This rest was permanent, the animal remaining, if accidents were excluded, upon its equator till it died. The question, then, here seems to resolve itself simply into this: Is the mechanism of the pedicels so constructed as to ensure that their serial action shall always take place in the same direction? For if it can be shown that their serial action may take place indifferently in either direction, it would follow that the persistency with which the partly shaved *Echini* continue reared upon their equators, is the expression of some stimulus (such as a sense of gravity) continuously acting upon some central apparatus, and impelling the latter to a continuous, though fruitless, endeavour to co-ordinate the absent pedicels. If the pedicels are able to act serially in either direction, there is no more reason why a partly shaved *Echinus* should remain permanently reared upon its equator, than that it should remain permanently inverted upon its pole; and therefore the fact that in the latter position the pedicels set about an immediate rotation of the animal, while in the former, and quite as unnatural position, they hold the animal in persistent stasis—this fact tends to show that the righting movements of the pedicels are something more than serial. Thus the whole question as between the two hypotheses amounts to whether the pedicels are able to act serially from oral to aboral pole. Observation has shown us that they are so, for we have seen *Echini* spontaneously rear themselves from their normal position on the oral pole, to the position of resting upon their equators. Further, as additional evidence that the righting movements are at least assisted by some centralising influence, is the fact that when the evolution is nearly completed by the pedicel-rows engaged in executing it, the lower pedicels in the other rows become strongly protruded and curved downwards, in anticipation of shortly coming into contact with the floor of the tank.

But, on the other hand, there is evidence to show that the action of the pedicels in executing this manœuvre, although, as we have seen, in some measure, is not exclusively dependent upon this centralising influence; and we found that the centre from which this influence proceeds is the nerve-ring that surrounds the lantern. For when this is removed, the following results are produced. The pedicels have their spontaneity impaired, though not destroyed—the animal still continuing to crawl, but only feebly, and no longer in a determinate manner, frequently changing its direction of advance, and showing a marked tendency to rotate upon its vertical axis. Moreover the echinus is now no longer able to escape from injury, but when stimulated crawls indifferently in any direction. Thus, removal of the nerve-centre seriously impairs the activity of the pedicles, and totally destroys their co-ordination. Yet when specimens so mutilated are inverted, one out of every four specimens is able to right itself. This, however, is only done with much difficulty and after a long time, so that, under these circumstances, the execution of this manœuvre seems to be just barely possible. Still the fact of its being possible at all proves that the integrity of the nerve-centre is not absolutely essential to its performance. Therefore, as experiment has failed to reveal to us any other general nerve-centre in the animal, and as even a segment of the animal containing only a single row of pedicels is in many cases able to perform this manœuvre, we conclude, as already stated, that the action of the pedicels is partly of a-serial character, though largely assisted by the co-ordinating influence that emanates from the nerve-centre.

The effect of this operation upon the spines and pedicellariæ still remains to be considered. No effect at all is produced upon the pedicellariæ; but upon the spines a profound influence is seen to be exercised. Their spontaneity, indeed, remains unimpaired, as does also the function which they share with the pedicellariæ of closing round any instrument of stimulation; likewise their power of responsive bristling all over the animal when any part of the animal is severely stimulated continues to be manifested as before, although for an hour or two after the operation this power is suspended by shock. But the general *co-ordination* of the spines is totally and permanently destroyed, for if the animal be placed upon a table and a spirit-lamp flame held against one side, although all the spines will manifest their bristling movements (if the period of shock has been allowed to pass away), they will no longer co-operate to remove the animal from the source of irritation. These facts prove, 1st, that the general co-ordination of the spines is wholly dependent upon the nerve-centre; 2nd, that the spontaneity and local reflex irritability are wholly independent of that centre—they depend entirely upon the external nerve plexus; and, 3rd, that the universal nervous connexions revealed in the bristling movements of the spines, and which, as shown by previously narrated experiments, depend upon the hypothetical internal nerve plexus, are themselves in nervous connexion with the nerve-centre. For only thus can we explain the long period of shock which removal of this centre entails upon the functions of this supposed internal plexus. Nevertheless, the fact that these functions are eventually resumed in the general bristling of the spines, proves that this general communication between the spines is maintained by the direct conductility of the supposed internal plexus, and is not of the nature of a reflex action, in which the nerve ring is concerned as a general centre for the responsive, as distinguished from the co-ordinated, action of the spines.

These results taken together prove that all parts of the nervous system of the Echinodermata are, in function as in structure, both central and peripheral, although the nerve-ring exercises a larger share of centralising influence than does any other part of the system.

II. "The Functional Relations of the Motor Roots of the Brachial and Lumbo-Sacral Plexuses." By DAVID FERRIER, M.D., F.R.S., Professor of Forensic Medicine, and GERALD F. YEO, M.D., F.R.C.S., Professor of Physiology in King's College. Received March 10, 1881.

The functions subserved by the plexiform arrangement of the nerves of the limbs, and the mode of distribution of the several roots of the brachial and lumbo-sacral plexuses, have been the subject of frequent speculation and of occasional experimental research; and the question is one of considerable physiological and pathological interest. A mere naked-eye examination of the mode in which the roots unite to form the larger trunks allows of an approximate determination of the possible roots of each trunk; and by more minute dissection and maceration in dissociating liquids, as has been done by W. Krause in the case of the brachial plexus ("Beiträge zur Neurologie der Oberen Extremität," 1865), the constituent fibres of the nerve-trunks may be determined with greater precision.

But it is obvious that anatomical dissection, however minute, is unable to discriminate between the sensory and motor constituents of the nerve-trunks, or to indicate their functional relations and distribution. The only possible methods by which this can be arrived at are by determining the effects of excitation or destruction of the individual roots of the plexus.

Both methods have been employed by different investigators in the case of several of the lower animals.

Johannes Müller and Van Deen experimented on the crural plexus of the frog. From these, as well as from a consideration of the results of Kronenberg's experiments referred to below, Müller came to the conclusion that "the plexuses of nerves seem to be destined, as far as their motor power is concerned, to convey to each muscle fibres from different parts of the brain and spinal cord. . . . The plexuses may also be intended to intermingle the sensitive and motor fibres in accordance with the wants of the parts to which the nerves are distributed" (Müller's "Physiology," translated by Baly, vol. i, p. 682). Kronenberg ("Plexuum Nervorum Structura et Virtutes," Berol, 1836, quoted by W. Krause, *op. cit.*) found that by mechanical or electrical stimulation of the roots of the brachial plexus in the rabbit, almost every muscle of the limb was thrown into action by each. His conclusions as to the functions of the plexus were essentially the same as those quoted from Müller.

A similar view was maintained by Bartolomeo Panizza, from the results of experiment on the crural plexus of frogs and goats.

("Richerche Sperimentali Sopra i Nervi," Lettera del Professore Bartolomeo Panizza al Professore Maurizio Bufalini. "Annali Universali di Medicina," December, 1834. Translated in "Froriep's Notizen," No. 945, March, 1835.)

Panizza found that section of one root caused only temporary weakness of the limb as a whole, a weakness which increased in proportion to the number of roots divided; but there was no complete paralysis till the last root was cut. He was of opinion that the various roots had a community of function, the whole forming a solidarity, but each by itself capable of maintaining the functions in their integrity.

The researches of Peyer ("Zeitschrift für Rationelle Medicin," N. F., Bd. iv, 1854) on the peripheric distribution of the sensory and motor roots of the brachial plexus in the rabbit were of greater precision than those of Kronenberg.

Peyer cut the roots of the plexus and ascertained, by exposure and dissection, which muscles were made to contract by weak electrical stimulation of each root. He also determined the region of distribution of the sensory roots by cutting all the roots save one, and then ascertaining in which area cutaneous stimulation still excited reflex movements.

The results obtained by Peyer as to the distribution of the motor roots differ considerably from those of Kronenberg, and he gives, in a tabular form, the respective muscles related to each root, from the fifth cervical to the first dorsal.

He found that most muscles received fibres from more than one root, and that the muscles supplied by each root did not fall into such simple groups as extensors, flexors, &c., but were more complicated. The muscles nearer the hand were supplied by the roots nearer the dorsal part of the cord. As regards the sensory distribution, the rule was that the sensory roots were distributed to the cutaneous surfaces overlying the muscles supplied by the corresponding motor roots.

Peyer's results were, in the main, confirmed by the researches of W. Krause ("Beiträge zur Neurologie der Oberen Extremität," 1865). Krause adopted the method of dividing the individual roots and then tracing the lines of degeneration in the sensory and motor nerves, according to the Wallerian law. He gives a tabular view of the peripheric distribution of the various roots, sensory and motor, in the work quoted, and again, with slight variations, in his work on the anatomy of the rabbit ("Anatomie des Kaninchens," 1868, p. 247).

Krause also made one interesting experiment on a monkey (*Macacus cynomolgus*). He divided the sixth and seventh cervical nerves, and ascertained that no degeneration ensued in the ulnar or median sensory nerves of the hand. He concluded, by analogy from the ascertained distribution in the rabbit, that the ulnar and median digital nerves were derived from the first dorsal and eighth cervical nerves respectively.

Passing from these experimental researches in the lower animals to observations in man, we have a fact of considerable significance in reference to the functional relations of the roots of the brachial plexus, which was first pointed out by Erb ("Diseases of the Peripheral Cerebro-Spinal Nerves," in Ziemssen's "Cyclopædia of the Practice of Medicine," vol. xi, p. 561). By faradisation over the brachial plexus, at a point corresponding with the exit of the sixth cervical nerve from between the scaleni muscles, the deltoid, biceps, brachialis, and supinator longus can be thrown into simultaneous contraction. At the same time, he says it is difficult to avoid the musculo-spiral nerve, which can also, however, be separately excited. We have ourselves found extension of the wrist a constant accompaniment of the action of the above muscular group when it is at all distinct. From the collocation of the muscles affected in atrophic spinal paralysis, it has been ably argued by E. Remak ("Zur Pathogenese der Bleilähmungen, Archiv für Psychiatrie," 1876; and "Ueber die Localisation Atrophischer Spinallähmungen und Spinaler Muskelatrophien," *ibid.*, 1879) that functionally related or synergic muscles are represented together in the anterior horns of the spinal cord; and he indicates, more or less tentatively, the probable position of the centres of certain brachial and crural muscular groups in the cervical and lumbar portions of the cord respectively.

With the view of throwing light on these various questions by physiological researches, which may be regarded as almost directly applicable to man, we have made a series of experiments on the motor roots of the brachial and lumbo-sacral plexuses in monkeys.

The brachial plexus in the monkey corresponds in its constitution, configuration, and distribution almost exactly with that of man. It is formed by roots from the fourth, fifth, sixth, seventh, and eighth cervical, and the first dorsal nerves. These unite and form the larger nerve-trunks in essentially the same way as in man. The phrenic nerve, however, arises from the third and fourth cervical, and does not, so far as we have examined, receive a branch from the fifth, as is usually the case in man.

The lumbar and sacral plexuses, however, do not at first sight appear to correspond, at least as regards the origin of the roots which enter into their composition. This is owing to the fact that there are seven lumbar vertebræ in the monkeys we have examined. If, however, we detach the first lumbar vertebra and add it to the dorsal, so as to make thirteen dorsal vertebræ, and count the last lumbar as the first sacral, the harmony, as regards the mode of distribution of the several roots, becomes complete. And, in one case which we examined, there were rudimentary ribs attached to the first lumbar vertebra. It seems, in fact, warrantable to make the constitution of the lumbar and sacral plexuses a means of determining to which group the variable dorsal, lumbar, and sacral vertebræ in monkeys should be referred.

The mode of operation carried out by Dr. Yeo was as follows:—The animals were deeply narcotised with chloroform, the vertebræ over the cervical or lumbar nerves exposed, the arches removed so as to expose the cord. The dura mater was opened, and in the case of the cervical and dorsal nerves the posterior roots were cut in order to avoid all reflex movements on stimulation. In the case of the roots forming the cauda equina this was not necessary, as the anterior and posterior roots run a long separate course, and can easily be isolated. The motor roots were stimulated with closely approximated needle electrodes by means of the induced current of Du Bois Reymond's secondary coil. A minimal current, barely perceptible on the tongue, and just sufficient to produce distinct action, was employed, the distance of the secondary coil varying from 20 centims. or less, according to the degree of excitability of the nerves, which is liable to considerable variation at various stages. Every precaution was taken to insulate the roots, and unavoidable diffusion was more or less eliminated by frequent repetition and uniformity in the results. Occasionally, in the case of the brachial plexus, where the clear exposure of the roots is somewhat difficult, we used as one pole a flat electrode over the sacrum as a neutral point, and stimulated the root by hooking it up on a curved needle, which formed the other pole of the circuit. The roots were stimulated either inside or outside the sheath, according to where they were most conveniently reached.

In observing the effects of stimulation, we directed our attention more especially to the resultant muscular combination rather than to the mere number of the muscles thrown into action. It will be seen that this is of no little importance, as the actions excited are all complex co-ordinated movements of great significance. It is very difficult, where so many events are occurring simultaneously, to analyse each muscular combination into the individual factors at work; and therefore the muscles which we state to be in action are those which we have collected from various observations, the muscles being felt or partially exposed where it was difficult to be sure of their co-operation otherwise. But we would not exclude, except when so stated, the co-operation of other muscles which were necessarily removed from actual observation, except by dissection.

But we believe that these may be fairly determined by anatomical considerations and by ascertained principles respecting the physiology of movements applied to the individual actions described.

We have experimented seven times on the brachial plexus, once unsuccessfully; and six times on the lumbo-sacral plexus, twice with only partial success on two roots.

The resultant actions have been very uniform, though occasionally incomplete, any variations of importance being specially indicated.

The Brachial Plexus.

Comprising the roots of the first dorsal, and the eighth to the fourth cervical.

First Dorsal.—Adduction of the thumb, and *flexion of the fingers at the metacarpo-phalangeal joints.*—The distal phalanges are slightly extended, and the fingers spread. The transverse diameter of the hand is diminished, and the dorsal aspect rendered more convex.

The action is that of the intrinsic muscles of the hand. Along with the action in the hand there is also contraction of the muscles on the same side of the neck, causing the head to be drawn towards the shoulder.

In our first two experiments this action was more or less complicated by that described under the eighth cervical, due without doubt to imperfect isolation of the irritation.

Eighth Cervical.—A complex action comprising *firm closure of the fist* (intrinsic muscles and long flexors of fingers and thumb), *pronation and flexion of the wrist* (to the ulnar side), *extension of the forearm with retraction of the upper arm* (long head of the triceps specially in action). The *extensor muscles* on the back of the forearm generally were specially observed in two instances to be rigid also.

The action here described may be exactly imitated by pulling some object hanging in front downwards and towards the hip, or by drawing a scimitar from heel to point through some object lying in front.

The muscles involved imply stimulation of nerves conveyed in the ulnar, median, and musculo-spiral.

The pectoralis major seems also to co-operate in this movement in man, but it was not specially observed in our experiments, though the shoulder was observed to be depressed. This would imply also the internal anterior thoracic, which anatomically is related to the cord formed by the eighth cervical and first dorsal.

Seventh Cervical.—The *upper arm is adducted, rotated inwards* and *retracted*, and the *forearm extended* so as to bring the dorsum of the hand against the rump, the *wrist and fingers flexed* (at their second phalanges), so as to bring the tips of the fingers towards the radial side and against the rump.

The action here described is the *scalptor ani* movement, and involves the co-operation of numerous muscles. The teres major, latissimus dorsi, and subscapularis appeared to be in action. The pectoralis major was noted in one instance also. The triceps was also observed to be contracting, and also the long flexors of the fingers.

These muscles would indicate stimulation of nerve-fibres contained in the subscapulars, musculo-spiral, and median—possibly also the external anterior thoracic to the pectoralis major.

Sixth Cervical.—The upper arm is *adducted and retracted*, the *forearm extended and pronated*, the *wrist flexed*, and the palm of the hand brought against the pubes.

This movement generally occurs quickly, and the palm of the hand is brought smartly backwards towards the middle line.

It seems to be the action which, if the hands were the fixed point, would raise the body upon a trapeze or branch.

We noted the contraction of the pectoralis, the latissimus dorsi, triceps, and flexors of carpus. The pronators were also evidently in action, though not visible.

These muscles imply stimulation of the external anterior thoracic, long subscapular, and branches of the musculo-spiral and median nerve.

Fifth Cervical.—The *upper arm is raised upwards and inwards*, the *forearm flexed and supinated*, the *wrist and basal phalanges extended.* The fingers assume a claw position with their distal phalanges bent.—The result of this action is to bring the hand up to the mouth. Among the muscles in action we specially noted the deltoid, its clavicular portion more particularly, the serratus magnus, the flexors of the forearm—biceps, brachialis anticus, and supinator longus. Also the extensors of the wrist and basal phalanges.

These muscles imply stimulation of nerves conveyed by the circumflex, musculo-cutaneous, and musculo-spiral, and apparently also the median (long flexors of fingers).

Fourth Cervical.—The *shoulder and upper arm are raised upwards and backwards*, the *forearm flexed and supinated* (*and the wrist extended*).—The action here is in other respects similar to that of the fifth cervical, except in the raising the arm upwards and backwards. The muscles we observed in action were the deltoid, the rhomboid, the supra- and infra-spinatus muscles, the flexors of the forearm and extensors of the wrist, though occasionally the last were not observed.

During stimulation of this root respiration ceases from *spasm of the diaphragm.*

The action described implies stimulation of fibres conveyed by the rhomboid, supra-scapular, circumflex, musculo-cutaneous, and musculo-spiral nerves, and also of the phrenic.

We would likewise note a fact which may require further investigation, viz., that in one case in which we specially directed attention to the pupil, during stimulation of the motor roots from the first dorsal up to the fourth cervical, no action on the pupil was observed, though the usual movements of the limb occurred. This is a fact which is of importance in reference to the question of a cilio-spinal centre in the lower cervical region of the cord.

At present we merely note the above fact, reserving the subject for future inquiry.

The Lumbo-Sacral (Crural) Plexus.

The roots which supply motor fibres to the lower extremity comprise the first sacral, seventh, sixth, fifth, and fourth lumbar in the monkey, corresponding, as already indicated, to the second sacral, first sacral, fifth, fourth, and third lumbar in man respectively.

Stimulation of the second sacral, and of the third and fourth sacral in the instance where we exposed these roots, gave rise to movements of the tail. We could not determine movements of the pelvic muscles.

First Sacral (second sacral in man).—*Adduction and flexion of hallux* (basal phalanx), *flexion of the proximal phalanges of the toes* with slight separation and extension of the distal phalanges. The tail also moves to the same side.—The action here is identical with that of the first dorsal in the brachial plexus, and is due to the intrinsic muscles of the foot.

Seventh Lumbar (first sacral in man).—*Flexion of the leg* (hamstrings), *plantar flexion of the foot* (sural muscles), *adduction of the hallux* and *flexion of the toes at the proximal phalanges* (as in first sacral) with the addition of *flexion of the hallux at the distal phalanx* (long flexor).—The thigh is slightly rotated outwards, so that the plantar aspect looks towards the middle line.

The tibial and peroneal muscles and long flexors of the fingers do not act.

The nerves in action are branches of the great sciatic and its internal popliteal divisions.

Sixth Lumbar (fifth lumbar in man).—*Rotation outwards of thigh* (which assumes a position midway between extension and flexion), *flexion of the leg with inward rotation*, so that the foot points inwards, *plantar flexion of the foot* with *flexion of the hallux and toes at their distal phalanges.* The outer edge of the foot is somewhat raised.—This complex action involves the co-operation of many muscles difficult to analyse. We noted action of muscles in the gluteal region, the hamstrings, sural muscles, long flexors, the tibialis anticus and posticus, the peroneal muscles, and also the extensors of the toes.

This involves stimulation of nerves from the trunk, and from the external and internal divisions of the great sciatic nerve.

Fifth Lumbar (fourth lumbar in man).—*Extension of the thigh, extension of the leg* and *pointing of great toe.*—The combined result is straightening of the whole limb directly backwards, and seems to be the movement which immediately precedes the lifting of the foot to take another step forward in the act of walking.

The muscles we observed in action are the gluteal, the adductors, extensor cruris, and the peroneus longus. This latter explains the pointing of the great toe by depression of the base of the first metatarsal bone; and at the same time the raising of the outer edge of the foot. The sural muscles did not appear to contract.

This apparently involves stimulation of nerves conveyed by the superior gluteal, anterior crural, obturator, and musculo-cutaneous branch of the external popliteal nerve.

Fourth Lumbar (third lumbar in man).—*Flexion of the thigh* on the pelvis and *extension of the leg.*—This brings the leg in the line straight forwards.

In addition to the ilio-psoas (evidently in action, though not visible), the sartorius, adductors, and extensor cruris were observed to contract. No action was observable in the muscles of the leg or foot.

The nerves involved are conveyed by the anterior crural and obturator trunks.

Stimulation of the *third lumbar* (second in man) caused contraction of muscles in the flank, but no action in the leg. The cremaster muscle was not observed, though its contraction should be expected here. Stimulation of the *second* (first in man) and of the *first* lumbar nerves caused contraction of some muscles in the flank and hypogastric region.

It will be seen that the movements which result from stimulation of the individual roots of the brachial and crural plexuses are not mere contractions, more or less strong, of various muscles (though many muscles are excited to contraction by more than one root, as previous experimenters have found), but a highly co-ordinated functional synergy in each case, as Remak has supposed.

The muscles thrown into action by each root are innervated in most cases by several nerve-trunks, whence it would appear that the plexiform junctions of the various roots are for the purpose of distributing the requisite motor fibres in different trunks to the various muscles engaged in each functional combination.

The result of section of each motor root would, therefore, be paralysis of the corresponding combination, not necessarily, however, of the individual muscles involved. For, as many of these are innervated by more than one root, the degree of paralysis of the muscles would depend on the degree of motor innervation by the root divided; and, therefore, while weakened, they might yet act in other combinations in so far as they were supplied by other roots. Such appears to us the real explanation of the fact stated by Panizza, that there was no absolute immobility of the limb in his experiments until every root was cut.

It is evident that the cervical and lumbar enlargements of the spinal cord are centres of highly co-ordinated muscular combinations. These (as Krause's researches on rabbits, compared with ours, would seem to indicate) may differ in different animals according to their habits and modes of activity.

The results we have obtained are capable of numerous physiological and pathological applications, but for the present we content ourselves with the above brief statement of some of the more obvious conclusions which they seem to justify.

III. "On the Histology and Physiology of the Pepsin-forming Glands." By J. N. LANGLEY, M.A., Fellow of Trinity College, Cambridge. Communicated by Dr. MICHAEL FOSTER, F.R.S. Received March 11, 1881.

(Abstract.)

This paper contains an account of observations upon *Rana temporaria*, *Bufo vulgaris*, *Triton tæniatus*, *Triton cristatus*, and *Coluber natrix*.

In these animals the changes which take place in the pepsin-forming glands under various conditions, viz., digestion of varying amounts of food, attempted digestion of indigestible substances, as sponge, and fasting, were noted.

Omitting the pyloric glands, which in these animals form only a minimal amount of pepsin, all the pepsin-forming glands present certain phenomena in common. In all, the living gland-cells contain granules, which diminish during digestion. In each the amount of pepsin* contained by a definite weight of the gland-bearing mucous membrane is proportionate to the amount of granules contained by the gland-cells. An increase or diminution of the cell-granules, in whatever way it is brought about, is accompanied by a corresponding increase or diminution in the pepsin-content of the cells. Further, in different parts of the pepsin-forming region the amount of pepsin present is proportional to the granularity of the cells.

Hence we may conclude that the granules consist wholly, or in part, of pepsin or of some substance capable of giving rise to pepsin.

In some of these animals certainly, and probably in all, the fresh glands contain only a minimal quantity of pepsin, but, on the other hand, a large quantity of a substance from which pepsin can be obtained, *i.e.*, zymogen. It follows, then, that in the previously mentioned estimation of pepsin, the pepsin found resulted from the splitting up of zymogen, and consequently that the amount of zymogen contained by the gland-cells is proportional to their granularity.

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* The dried mucous membrane was extracted with one hundred times its weight of hydrochloric acid, 0·2 per cent. Grützner's colorimetric method was used in making the estimations.

Hence we may conclude that the granules consist not of pepsin, but wholly or in part of zymogen.

The processes in these pepsin-forming glands, then, closely resemble the processes which go on in the pancreatic gland. The cell-protoplasm stores up zymogen. At the moment of secretion the zymogen is converted into ferment, and probably other organic substances found in the fluid secreted.

In all the glands, with the exception perhaps of those of the snake, the cells, which diminish somewhat in size as well as in granularity in the first period of digestion, recover more or less completely their normal size and their normal granularity during the latter period of digestion. In other words, at a time when the using up of granules is still proceeding, fresh granules are formed; and, since the granules result from protoplasmic metabolism, we may conclude, bearing in mind the increase in size of the cells, that the protoplasm is also growing.

Thus in any gland-cell, during at any rate the greater part of digestion, three processes are going on at the same time, viz., the growth of protoplasm, the formation of zymogen by the protoplasm, the conversion of zymogen into secretory products. There are certain reasons which make it in a high degree probable that these three processes go on during the whole of the digestive period. Under certain conditions, there is a preliminary increase in the size of the cells and the granules before the normal decrease sets in. When the decrease in the size of the cells does set in, it is not sufficient to allow us to suppose that there is no growth of protoplasm; the casting out of the granules which have disappeared would, if there were no protoplasmic growth, leave the cell much smaller. The analogy of the change in the mucous salivary glands during secretion affords an instance in which the cell-protoplasm increases steadily during activity.

Lastly, as regards the granules, it is clear that there might be a fairly rapid formation of granules without any obvious change in the cell, provided the using up of granules went on at an equal rate.

On the whole, then, it may be, I think, fairly concluded, that, from the beginning to the end of the digestive period, the three processes mentioned go on. If this be the case, the different appearances of any cell as regards size and granularity depend upon the relative rates with which the three processes proceed at different times.

When the amount of change which takes place in the gland-cells of different animals during digestion is compared, very wide differences are found. The most striking comparison is perhaps afforded by the glands in *Triton tæniatus* and those in *Triton cristatus*. At the fourth hour of digestion, when the cells have apparently secreted approximately proportional amounts of pepsin, *i.e.*, when they have used up an approximately proportional amount of granules, the observable diminution of granules in the gland-cells of *Triton cristatus* is enormously less than the observable diminution of granules in the gland-cells of *Triton tæniatus.*

This can, I think, only result from the formation of granules proceeding more hand-in-hand with the using up in the former than in the latter.

This and other similar facts lead us to conclude that the differences in the amount of histological change which takes place in the pepsin-forming glands of different animals, is the consequence of the three changes above-mentioned proceeding in each animal at different relative rates.

It is well known that in various glands, typically the pancreas, the granules are found during secretion aggregated around the lumina, the outer portions of the cells being non-granular and homogeneous. The cause of this appearance is, no doubt, in part due to a more rapid growth of protoplasm in the outer than the inner portions of the cells; in part, also, I think it is due to the granules being moved by the protoplasm towards the lumen. It is necessary, I think, to assume such a transference of granules, since neither the unequal growth of protoplasm, nor, what might also be imagined, an unequal using up of granules in the two regions of the cells, is sufficient to account for the changes which take place. Since the granules before they disappear become smaller, they will be smaller in that part of the cell in which they are being used up most actively. But, as a matter of fact, the granules are equally affected throughout the cell, so that the outer clear zone cannot arise from a more rapid using up of granules in that part of the cell. As to the more rapid growth of protoplasm, it fails to explain how the granules, which in the first stage of digestion become few and scattered in the outer half of the cell, can in the latter stages of digestion be arranged in a dense mass around the lumen; it leads, moreover, to the very improbable hypothesis that when the granules have entirely disappeared from the cells, the cell protoplasm has completely regrown from the periphery.

The pepsin-forming glands which we are considering in this paper, offer some other interesting forms of cell activity. In some no distinction of zones occurs, the granules become smaller and less frequent in all parts of the cells, without any alteration in their relative distribution; in others there is formed a small non-granular zone in the inner part of the cells. This inner non-granular zone, though obvious enough in certain states of the glands, does not reach the proportions which the outer non-granular zone attains in the pancreas and other similarly constructed glands.

Further, we find, in these glands, all stages of transition between the three types of cell-change in activity which have just been mentioned. Thus, the œsophageal glands of the frog form a large outer non-granular zone, the oxyntic glands* of *Triton tæniatus*, form normally a less developed, though distinct, outer zone, this being less in the posterior than in the anterior oxyntic glands. In the glands of *Triton cristatus*, the outer zone is reduced to a minimum, or is represented only by a greater thinning out of granules in the peripheral part of the cells; the scanty œsophageal glands of the toad show a bare trace of a similar difference in the two parts of the cell. In the greater number of the oxyntic glands of the snake, and in the anterior oxyntic glands of the stomach of the toad, neither an outer nor an inner zone is formed, but in passing backwards to the posterior oxyntic glands in both of these animals, an *inner* non-granular zone gradually becomes obvious. The inner zone reaches its greatest development, so far as I have observed, in the posterior oxyntic glands of the frog.

In this brief account the general conclusions which can, I think, be drawn from the study of the pepsin-forming glands are only given, I have made no reference to the important papers of Heidenhain, Grützner, Swieçicki, Nussbaum, Partsch, and others on the same subject. I have discussed their work in the fuller paper, which contains the details on which the conclusions here given are based.

March 31, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Coefficients of Expansion of the Di-iodide of Lead, PbI_2, and of an Alloy of Iodide of Lead with Iodide of Silver, $PbI_2.AgI$." By G. F. RODWELL, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Professor A. W. WILLIAMSON, For. Sec. R.S. Received March 10, 1881.

(Abstract.)

The author having referred to his previous papers on the coefficients of the iodides of silver and mercury, and of certain chlorobromidides of silver, continues the same methods of determining the coefficients of the iodide of lead, and of an alloy of the iodides of lead and silver. Various alterations in the apparatus, which by the diminution of friction and other means have rendered it much more delicate than before, are described. The iodide of lead was found to possess three coefficients of expansion; the first for temperatures between 0° and 205° C.—

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* I propose to use the term "oxyntic" (ὀξύνειν, to make sour, to acidulate) for those glands which are called by different observers by the inappropriate names "fundus," "peptic," or "rennet" glands.

·00008317 cubical expansion for 1° C.;

the second, a very high coefficient, between 205° C. and 253° C.—

·0006378;

and the third, for temperatures between 253° C. and the fusing point—

·000180.

The volumes at the different temperatures are given and tabulated, and the curve of expansion is plotted.

The lead silver iodide, $PbI_2.AgI$, is next examined.

It contains in 100 parts:—

Iodide of lead	66·206	Lead	29·7449
Iodide of silver	33·794	Silver	15·5642
		Iodine	54·6909
	100·000		100·0000

Between 0° and 118° C., it slowly expands when heated, with a cubical coefficient for 1° C. of

·0000306.

Then for a few degrees (118—124° C.), it simply absorbs heat without contracting or expanding. On reaching a temperature of 124° C., the mass commences to contract on further heating, and this continues until a temperature of 139° C. is attained. Details of the contraction are given, and the curve of contraction is compared with that of the iodide of silver. Between 139 and 144° C., heat is again absorbed without change of volume; and above 144° C. the alloy expands somewhat rapidly, with a coefficient of

·0001150.

The volumes at various temperatures between 0° and the fusing point are given, and are shown in a curve.

The following points are noted in regard to the alloy:—

1. It possesses a similar density at three different temperatures, as at 0° C., 130° C., and 282° C.

2. Although it contains only 33·794 per cent. of iodide of silver, it contracts as considerably *during heating* as the iodide of silver itself.

3. While the iodide of silver commences its contraction at 142° C., and finishes it at 145°·5 C., the alloy commences to contract 18° C. lower (viz., at 124° C.) and finishes 6°·5 C. lower (viz., at 139° F.).

4. The chlorobromiodides of silver ("Proc. Roy. Soc.," vol. 25, p. 292) also began to contract on heating (an effect which, of course, we must attribute solely to the presence of iodide of silver), at 124° C., but they finished at 133° C.

5. The harsh sounds emitted by the alloy during cooling, and the tremors simultaneously propagated through the mass, prove that violent molecular agitation is going on at such time as the iodide of silver is passing from the amorphous plastic condition to the brittle crystalline condition, within the mass of the iodide of lead.

6. The fusing point of the alloy is 125° C. lower than that of the iodide of silver, which constitutes one-third of its weight, while it is only 19° C. higher than that of the iodide of lead, which constitutes two-thirds of its weight.

7. If the lowering of the fusing point (also markedly apparent in the case of the chlorobromiodides of silver) is due to the fact that similar particles of matter attract each other more powerfully than dissimilar, and hence when the particles of two bodies are mutually diffused, the attraction becomes less, and the molecular motion is consequently more readily assimilated, the same cause may serve to explain the commencement of the phase of contraction on heating the alloy at a temperature of 18° C. lower than the substance to which it owes this property.

The lead silver iodide alloy is finally compared with a chlorobromiodide of silver, which latter, although it contains 8 per cent. more of iodide of silver than the lead silver iodide alloy, undergoes a contraction on heating, which is more than twenty times less, although in both cases we must regard the effect as solely due to the iodide of silver.

II. "Permanent Molecular Torsion of Conducting Wires produced by the Passage of an Electric Current." By Professor D. E. HUGHES, F.R.S. Received March 17, 1881.

In a paper on "Molecular Electro-Magnetic Induction," presented to the Royal Society March 7, 1881 (p. 524), I gave a description of the induction currents produced by the torsion of an iron wire, and the method by which they are rendered evident. The electro-magnetic induction balance there described is so remarkably sensitive to the slightest internal strain in anywise submitted to it, that I at once perceived that the instrument could not only determine any mechanical strain such as torsion or longitudinal stress, but that it might indicate the nature and cause of internal strains. Upon putting the question to it, does the passage of electricity through a wire produce a change in its structure? the answer came, it does, and that to a very considerable extent; for an iron wire adjusted to perfect zero, and which would remain free from any strain for days, becomes instantaneously changed by the first passage of a current from a single cell of Daniell's battery; the wire has now a permanent twist in a direction coinciding with that of the current, which can be brought again to zero by mechanically untwisting the wire, or undoing that which the passage of electricity has caused. Before describing the new phenomenon, I will state that the only modification required in the apparatus, is a switch or key by means of which the telephone upon the wire circuit is thrown out of this circuit, and the current from a separate battery of two bichromate cells passed through the wire alone, at the same time, care being taken that no current passes through the coil, but that its circuit should remain open during the passage of the electric current through the wire under observation; an extra switch on this circuit provides for this. The reason for not allowing two currents to react upon each other, is to avoid errors of observation which may be due to this cause alone. When, however, we take an observation, the battery is upon the coil and the telephone upon the wire alone; an experiment thus consists of two operations. First, all external communications interrupted, and an electric current passed through the wire; and, second, the electric current taken off the wire, and all ordinary communications restored. As this is done rapidly by means of the switches, very quick observations can be made, or if desired the effects of both currents can be observed at the same instant.

Now, if I place upon the stress bridge a soft iron wire $\frac{1}{2}$ millim. diameter, 25 centims. long, I find, if no previous strain existed in the wire, a perfect zero, and I can make it so either by turning it slightly backwards or forwards, or by heating the wire to a red heat. If I now give a torsion to this wire, I find that its maximum value is with 40° torsion, and that this torsion represents or produces electric currents whose value in sonometric degrees is 50; each degree of torsion up to 40 produces a regular increase, so that once knowing the value of any wire, we can predict from any sonometric readings the value in torsion, or the amount of torsion in the opposite direction it would require to produce a perfect zero.

If now I place this wire at zero, and thus knowing that it is entirely free from strain, I pass an electric current through it, I find that this wire is no longer free from strain, that it now gives out induction currents of the value of 40, and although there is no longer any battery current passing through this wire that the strain is permanent, the outside coil neither increasing or diminishing; the internal strain it has received by the passage of an electric current through the wire, upon giving a torsion to the wire in one direction, I find the inductive force increase from 40 to 90, but in the other direction it is brought to zero, and the amount of torsion some 35° required to bring the wire again to zero represents exactly the twist or strain that had been produced instantaneously by the passage of an electric current. If I repeat the experiment, but reverse the battery current sent through the wire, I find an opposite twist of exactly the same value as previously, and that it now requires an opposite torsion to again bring the wire to zero. It is not necessary, however, to put on an equal opposite torsion on wire to bring the currents to zero, for, as I have shown in my late paper, the sonometer not only allows us to measure the force and indicate its direction, but allows us to oppose an equal electric current of opposite name, thus producing an electrical zero in place of the mechanical one produced by torsion.

Evidently here there has been a sudden change in the structure of the wire, and it is a twist which we can both measure and reproduce. The question at once becomes, has a molar twist been given to the wire such as would be detected by the arm or free end of the wire, or a molecular change leaving no trace upon its external form of what has passed?

It will be found that, notwithstanding that it requires some 40° of torsion to annul the effects of a passage of an electric current, no visible movement nor any tendency of the free end to turn in the direction of the twist it has received can be observed. I believe, however, to have noticed a slight tremor or movement of half a degree, but as I could not always reproduce it, and as it is so slight compared with the 40° of internal twist, I have not taken it into account, for if the wire is firmly fastened at both ends, no molar torsion being possible, except an elastic one, which would instantly spring back to zero, the current on passing produces its full effects of twist and it is permanent. Thus, the molecules have in some extraordinary way rearranged themselves into a permanent twist, without the slightest external indication of so great a change having taken place. An equally remarkable change takes place in aid of, or against (according to direction of current) an elastic permanent strain. Thus, if I first put the wire under 40° right-handed permanent torsion, I find its value to be 50. Now, passing the positive of battery through its free end, and negative to fixed end, the induction currents rise at once in value to 90; if, now, the negative is momentarily passed through the free end and positive to fixed end the induced currents at once fall to 10, and these effects remain, for on taking off the elastic torsion the wire no longer comes to zero, but has the full twist value produced by the current.

Tempered steel gave only one or two degrees against 50 for soft iron, but supposing this might be due to its molecular rigidity, I carefully brought the wire to zero, and then observed the first contact only. I found, then, that the first contact gave a value of 40, but the second and following only one or two. By bringing the wire back to zero by a momentary touch with a magnet, a continued force of 40, or if constant reversals were used instead of a simple contact, there was constant proof of a similar great molecular change by the passage of a current in steel as well as iron.

I can find no trace of the reaction of the wire upon the magnetism of the earth, as in all positions the same degree of force was obtained if great care is take that the wire is absolutely free from longitudinal magnetism. There is, however, a slight reaction upon its own return wire if brought within 1 centim. distance of the wire, and this reduces the twist some 10°. The maximum effects are obtained when the return wire is not nearer than 25 centims.; thus, the action is not one produced by a reaction, but by direct action upon its internal structure.

Copper and silver wires so far show no trace of the action. I believe, however, that a similar strain takes place in all conductors, and I have obtained indirectly indications of this fact; in order, however, to verify this, would require a different method of observation from the one I have described, and I have not yet perfected the apparatus required.

It seemed probable that if I approached a strong permanent magnet to the wire, I should perceive a twist similar to that produced by the passage of a current; but no such effects were observed, but it has a most remarkable effect of instantly bringing to zero a strain produced by the current, and, no matter which pole, the effect was the same. Thus, a strain of 50°, which remains a constant, instantly disappears upon the production of longitudinal magnetism, and I have found this method of reducing an iron wire to zero of strain far more effective than any other method yet tried, such as vibrations, heat, twisting, &c.

It will be seen from this that the molecular arrangement set up by magnetism is very different from that produced by the passage of an electric current. It evidently has a structure of its own, else it would not have instantly destroyed the spiral strain left by the passage of electricity if it had not taken up a new form, as rendered evident in the longitudinal magnetism, which we could at once perceive on the wire. This question, however, belongs to a separate investigation, and I hope the apparatus will aid me later in throwing some new light upon this subject.

Another method of reducing the wire to zero, after the passage of a current, is to keep the wire in a constant state of vibration. It requires in time about one minute to bring it to zero, but if, on the contrary, I set the wire vibrating during the passage of the current, the permanent twist becomes greater and more difficult to reduce to zero.

If a wire which has internal strains is heated to redness, these strains almost entirely disappear, and I can thus reduce by heat a strain which a current had produced, but heat, whilst allowing of greater freedom and motion of its molecules, does not prevent an internal strain being set up, for whilst heat can reduce the wire to zero, after the passage of the current, the effects are increased. If, during the time that the wire is at a red heat, the current is passed in the same time, and at the same instant we take off the current and the external heat, the wire when cold will be found to have a higher degree of strain than previously possible with the wire when cold.

We have seen that both mechanical vibrations and heat can reduce the wire to a zero, but its action is very slow, several minutes being required; but the action of electricity in producing a permanent twist is exceedingly quick. I have found that a single contact, whose duration was not more than 0·01 of a second, was equal to that of a prolonged contact of several minutes, and magnetism was equally as quick in reducing this strain to zero. And it is the more remarkable when we consider the very great mechanical force required by torsion of the wire to untwist the strain produced in an instant of time by electricity.

The results I have given are those obtained upon soft iron wires of $\frac{1}{2}$ millim., but I have experimented with different sizes up to 3 millims. diameter. The results with 1 millim. diameter were quite as evident as the $\frac{1}{2}$ millim., but on the 3 millim. wire the strain was reduced to 25° instead of 50°, owing to the extreme rapidity and low electrical resistance, compared with my small battery wires. On a telegraph line, the wire of which is almost entirely of iron, there must be a very great strain set up, which, however, would remain a constant, except where reversed currents are used, and in this case a constant movement of the molecules of the wire must be the result.

I believe it to be most important that we should determine, as far as we can by experimental research, the nature of all molecular changes produced by electricity and magnetism, and in this belief I am happy in being able to bring this paper before the Royal Society.

III. "On the Tendinous Intersection of the Digastric." By G. E. DOBSON, M.A., M.B. Communicated by Professor J. D. MACDONALD, M.D., F.R.S. Received March 14, 1881.

The digastric muscle in man and in many other mammals consists as is well known, of an anterior and a posterior portion, united by a tendon, or having their limits defined by an intermediate tendinous intersection. In most mammals, however, this tendinous intersection is either very feebly defined, or it is altogether absent. What, then, is its meaning when present, and on what does its relative development depend?

The distinct tendon which exists between the anterior and posterior bellies of the digastric in man and in other *Primates*, might reasonably be supposed to be produced by tension from the aponeurotic loop, which attaches it to the greater cornu of the hyoid bone; but in many species of other orders a tendon or tendinous intersection is found where the connexion of this muscle with the hyoid bone is either very feeble, or distributed equally over the whole extent of its anterior belly, or obsolete; and in which, moreover, the direction of the intersection, where it traverses the substance of the muscle, is almost invariably oblique* and directed downwards, forwards, and inwards in a direction leading from and not towards the hyoid. The true significance of the tendinous intersection is to be found, as I shall endeavour to show, in the remarkable form assumed by the anterior belly of the muscle in certain species.

While engaged in the dissection of a specimen of that rare mammal. *Gymnura rafflesii*,† I was much struck by the peculiar form of the anterior belly of the digastric. About the middle of the muscle, at its narrowest part, it is traversed by an oblique tendinous intersection more marked internally, arising from the upper margin, which is continued inwards and slightly forwards from its lower and internal margin as a tendinous band across the mylo-hyoid muscle, to unite in front of the hyoid bone with the corresponding band from the opposite side. From the tendinous raphé thus formed, muscular fibres arise, which, extending forwards and inwards, cover the anterior three-fourths of the mylo-hyoid and part of the genio-hyoid muscles, and passing above the margins of the anterior bellies of the digastric muscles of opposite sides, are inserted with them and for some distance in front of them, into the rami of the mandible.

This horizontal muscular expansion, which takes its origin thus from the tendinous intersection and its median continuation, might also be described as a deep division of the anterior belly of the muscle, commencing at the tendinous intersection, and uniting with its fellow of the opposite side along the middle line between the jaws. Its posterior margin, the tendinous raphé, above described, is concave, quite free from the hyoid bone, but attached to a fascial aponeurosis, which passes backwards over it and the sterno-hyoid muscles.

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* In some species it is nearly straight, but this does not depend on attachment to the greater cornu of the hyoid bone, as I purpose showing further on.

† I owe the opportunity which has been afforded me of examining the anatomy of this rare species to the kindness of Mr. W. T. Blanford, F.R.S., whom I have also to thank for many specimens of Oriental Insectivora, which have furnished me with much valuable material in working out my systematic and anatomical monograph of the *Insectivora*.

On removing this muscular expansion, the mylo-hyoid muscles are found beneath, very thin, not extending half the distance between the hyoid bone and the symphysis menti.

In *Tupaia ellioti* the anterior bellies of the digastric also unite in the middle line between the jaws, but there is no separate superficial external lamina in direct continuation with the posterior belly as in *Gymnura rafflesii*. The intersecting tendon is narrower but more distinct, arises as in that species from the upper margin of the muscle, and is continued downwards, forwards, and inwards to unite with the corresponding tendon from the opposite side across the mylo-hyoid, precisely as in *G. rafflesii;* but, unlike its condition in that species, it is closely adherent to the muscle on which it lies, and is connected by a strong fibrous aponeurosis with the body of the hyoid bone, while the greater cornu receives also some tendinous fibres from the lower margin of the posterior belly. The united anterior bellies pass forwards, taking their origin from the tendinous raphé, and, separating slightly near the symphysis, are inserted into the rami of the mandible, and by a fibrous aponeurosis into the symphyseal angle. The mylo-hyoid muscles, nearly wholly concealed by them, are, as in *G. rafflesii*, feebly developed, and do not extend more than half their length.

In *Chiromys madagascariensis*, the anterior bellies of the digastric muscles are united between the jaws,* but in other Lemuroids, as also in man, they are separate, each consisting of "two thick fleshy bellies with a long and strong median tendon."† In *Loris gracilis* I found a strongly marked oblique tendinous intersection (which has been described as a rudimentary tendon),‡ connected by a fibrous aponeurosis with the greater cornu of the hyoid bone and also with the surface of the mylo-hyoid in front of it, so that here we have evidently a case of oblique tendinous intersection passing into tendon.

In *E. macrocephalus* and in *E. minor*, among the *Chiroptera*, I found the anterior bellies double, the deep lamina united and forming a horizontal muscular expansion extending backwards even behind the hyoid bone, but wholly unconnected with it, its posterior margin being connected laterally with the tendinous intersection and medially with a fascial aponeurosis loosely covering the sterno-hyoid muscles. It is especially interesting to note that here, where union of the muscles extends so far backwards, the tendinous intersection is vertical, or nearly so, and the direction of the fibres of the deep or internal lamina transverse, while those of the superficial external part spring from it at right angles to its direction and pass forwards to their insertion into the jaw at a short distance in front of their origin. Here then we have a vertical tendinous intersection unconnected with the hyoid bone. The mylo-hyoids are altogether absent, their places and part of their function being evidently taken by the united digastrics, which extend almost the whole length of the jaws; while their action on the hyoid is performed by the greatly developed genio-hyoid muscles.

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* Owen, "Trans. Zool. Soc.," v, p. 43.

† Murie and Mivart, "Trans. Zool. Soc.," vii, p. 18.

‡ By Van der Kolk and Vrolik, referred to by Murie and Mivart, *loc. cit.*

In the much larger but externally very similar species,* *Epomophorus franqueti*, the anterior bellies of the digastrics are single and not united between the rami of the mandible, and the mylo-hyoids are present and well developed; the tendinous intersection described above is, nevertheless, visible, and though much less defined than in *E. minor*, occupies precisely the same relative position, and has the same vertical direction.

In *Herpestes nipalensis* the digastrics are very large and united between the jaws; the tendinous intersection is nearly vertical and is continued inwards to unite with the corresponding band from the opposite side slightly behind the hyoid bone. The raphé thus formed is connected with the fascia covering the sterno-hyoid muscles; and the deep surface of the united bellies is inseparably united, along the middle line, with the very feebly developed mylo-hyoids which they conceal altogether.

In man, as an anomaly, the fibres of the two anterior bellies have been found blended with each other, and expanded into a muscular plane with considerable attachment to the jaws;† or the anterior belly has been found double, the second part uniting with its fellow in a median raphé below the chin, overlapping the mylo-hyoid muscle;‡ or the tendon of intersection not unfrequently is continued from one side across to the other, forming a zone immediately above the hyoid bone, to the body of which it is tied down by fibrous tissue; from the upper surface of this the anterior bellies arise, either *a*, distinct, or *β*, more rarely inseparable.§

It may be observed that this last described condition is precisely that of the same muscles in *Tupaia ellioti*, and is extremely interesting, as evidently indicating reversion to a state of the muscle, probably once normal in ancient mammalian types, and which is now represented in man and most other mammals by a fascial layer only—the *supra-hyoid aponeurosis*—which, springing from the tendons of the digastric muscles of opposite sides, extends between their anterior bellies, covering the mylo-hyoid and part of the genio-hyoid muscles, as the similarly placed muscular expansion in *Gymnura* and in *Tupaia*.

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* All the species of this genus agree very closely in certain external characters. (See " Catal. Chiropt. Brit. Mus.," p. 5.)

† Fleischmann, quoted by McWhinnie, "Lond. Med. Gaz.," xxxvii, p. 186.

‡ Wood, quoted by Macalister, " Trans. Roy. Irish Acad.," xxv, p. 33.

§ Macalister, *loc. cit.*

In the species of the genus *Erinaceus* (the only other genus included with *Gymnura* in the family *Erinaceidæ*), a superficial oblique tendinous intersection occupies precisely the same position as in *Gymnura*, but is much less developed; it extends from the upper margin of the muscle (which is nearly of the same calibre from its origin to its insertion), appearing on both surfaces as an oblique tendinous inscription, and on reaching its lower margin is continued inwards into a fascial expansion extending between the anterior bellies of the muscles of opposite sides, covering the mylo-hyoid muscle, but no muscular fibres arise from it, as in *Gymnura*. In *Centetes ecaudatus* and *Hemicentetes madagascariensis*, in *Pteropus edulis* and *medius*, and probably in all the species of that genus, in *Epomophorus franqueti* (referred to above), in *Megaderma lyra*, in *Phoca communis*, and in *Cavia aperea*, for instance, though the fascial expansion may be either absent or feebly marked, the transverse tendinous inscription is traceable, although its presence in some, as in *Centetes ecaudatus*, is indicated only by a faint superficial oblique line surrounding the muscle in the usual position.* In these species, in which the anterior bellies of the digastrics are not expanded and united between the jaws, the mylohyoid muscles are well developed.

The peculiar development of the anterior bellies of the digastric muscles in the species referred to above, and the relations between it and the tendinous intersection, demonstrate that whatever form this intersection may assume, whether that of a rounded tendon, tendinous band traversing the substance of the muscle, or feeble superficial tendinous inscription, it has evidently been originally developed in ancestral forms in the same manner, namely, as the point of attachment of a tendinous raphé extending inwards from the digastrics of opposite sides, and giving a fixed point from which the greatly expanded internal laminæ of their anterior bellies take their origin. In most species of mammals the anterior bellies are no longer united between the jaws, but the presence in many of a transverse tendinous intersection or inscription still indicates their original connexion.

While it appears evident that the original form and relations of the tendinous intersection of the digastric, if not in all mammals, at least in all those now showing a trace of tendinous inscription in that muscle, were such as we still find it in *Gymnura rafflesii* and *Epomophorus minor*, its modified form or apparent total absence in most species remains to be accounted for.

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* Professor Humphry thus describes it in *Phoca communis* and in *Cavia aperea* :—" Near its middle it presented in the seal a superficial transverse tendinous division which is probably the representative of the more distinct tendinous division in man ;" in the latter species, " where the muscle passes further forwards nearer to the symphisis, the tendinous division is still more marked, involving the greater number of the fibres, yet the muscle has nearly a straight course from its origin to its insertion."—" Journ. Anat. Phys.," ii, p. 320.

The leading modifications of the muscle, including what I consider its primary form, may be arranged as follows :—

A. With the anterior bellies united across the space between the rami of the mandible, the mylo-hyoid muscles feeble or absent.

a. Anterior belly double. Ex., *Gymnura.*
b. Anterior belly single. Ex., *Tupaia.*

B. With the anterior bellies separate, scarcely or not exceeding the posterior in calibre.

c. Tendinous intersection distinct. Ex., *Homo.*
d. Tendinous intersection rudimentary. Ex., *Erinaceus.*
e. Tendinous intersection absent. Ex., *Canis.*

And with respect to its attachments to the hyoid bone :—

C. Connected by ligament or by tendinous fibres with the hyoid bone. Ex., *Homo*, *Tupaia.*

D. Unconnected. Ex., *Canis*, *Gymnura.*

Where the digastric is directly connected by ligament with the greater cornu of the hyoid bone, as in man, other *Primates*, and the *Rodentia*, tension on the middle of the muscular mass would tend to the disappearance of the muscular fibres in the part directly acted upon, and so the tendinous intersection would alone remain forming a tendon ;* where, on the other hand, as in the dog and most other mammals, there is no such connexion, or the connexion is by fascial aponeurosis equally distributed between the sides of the anterior bellies of the muscles and the hyoid bone, as in *Erinaceus*, the character of the digastric would be mainly muscular throughout. Now, the special development of the central tendon, or its absence, appears to me to be directly related to the attitude of the head of the animal with respect to its body when engaged in swallowing its food. In the *Primates*, in most of the *Rodentia*, and in the arboreal *Insectivora*, the food is swallowed while the body is in the erect or semi-erect posture, and the head being bent forwards the cavity of the mouth is at right angles with the œsophagus.† In this position the mylo-hyoid and genio-hyoid muscles are relaxed, and cannot act efficiently in drawing the hyoid bone upwards and forwards, so as to allow the masticated mass of food to pass into the œsophagus ; this duty, therefore, partly devolves on the digastric muscles, and the strain on their central parts, as explained above, leads to the disappearance of muscular fibre in the neighbourhood of the tendinous insertion, which accordingly becomes a tendon uniting two fleshy bellies. On the other hand, in most of the Mammalia, the usual attitude is prone, and the cavity of the mouth, when the food is being swallowed, is in a line with the œsophagus, or nearly so; in such a position the mylo-hyoid and genio-hyoid muscles can act effectively in elevating and drawing the hyoid bone forwards, and deglutition is effected without the aid of the digastric muscles, which are accordingly simple and unconnected with the hyoid, as in the dog;* or, if intersected, the tendinous band is either the origin of a raphé continued inwards, as described in *Gynmura* and *Epomophorus* (which may be, as in these genera, wholly unconnected with the hyoid), or the rudiment of such in an ancestral form.

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* The intermediate condition is well seen in *Loris gracilis* and in *Tupaia ellioti*, as described above.

† The attitude of the head with respect to the body is of course mainly connected with the position of the foramen magnum, and the latter with the development of the brain.

IV. "Note on Protagon." By HENRY E. ROSCOE, LL.D., F.R.S. Received March 16, 1881.

In his communication to the Royal Society of January 6th last, on the subject of the presence or absence of potassium in protagon, Dr. Thudichum endeavours to raise an entirely false issue. The question I had to decide was not whether protagon contains a trace of potassium, but whether, to quote Dr. Thudichum's own words, it contains "no less than 0·76 per cent. of potassium." In the first instance I endeavoured to settle this matter by spectroscopic investigation, and employed two samples of protagon which had been prepared under Dr. Gamgee's direction in the course of the research of which he communicated the results to the Royal Society. In one of these samples, which had been four times crystallized, I was unable to detect any potassium; the quantity of the body at my disposal was however small, as the rest of the specimen had been employed in previous work, and Dr. Gamgee placed in my hands a large sample of protagon only twice crystallized, which had been prepared by Dr. Blankenhorn under his direction, and it was an analysis of this latter specimen which I communicated to the Royal Society. As stated, I estimated by spectroscopic means the amount of potassium present in 1 grm. of the substance to be $\frac{1}{20}$ of a milligram, that is, 0·005 per cent. To these observations of mine, Dr. Thudichum replied by a paper entitled "On the Modifications of the Spectrum of Potassium which are effected by the presence of Phosphoric Acid, and on the Inorganic

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* The difficulty experienced by a dog in swallowing when the head is bent forwards is well known to every one who has seen one attempting to swallow even a moderate sized morsel when seated erect in the familiar attitude known as "begging."

Bases and Salts which are found in combination with the Educts of the Brain." In this paper an attempt is made to prove that remarkable difficulties exist in obtaining the characteristic potassium line, and when even pure potassium phosphate is strongly heated. I do not deem it necessary to discuss with Dr. Thudichum, as bearing upon the detection of potassium by the spectroscope, the accuracy of such a statement as the following:—" Even a large bead of pure potassium phosphate, when ignited before the slit of the spectroscope never produces even at a white heat any such intense red potassium line as the smallest bead of potassium chloride," but shall merely state that, by numerous experiments, I have satisfied myself of the ease with which traces of potassium phosphate can be detected spectroscopically even in presence of a large excess of phosphoric acid; and that I am convinced thatthe estimate of the quantity of potassium present in the protagon reported upon in my first communication was a remarkably close approximation to the amount really present.

After Dr. Thudichum's reply, which seemed to leave it an open question, whether any reliance should still be placed upon his statement that protagon contained 0·76 per cent. of potassium, I determined to check that statement by a gravimetric analysis. Unfortunately a sufficient quantity of the sample first analysed was not available, and Dr. Gamgee supplied me with the remains of 130 grms. of protagon, twice crystallized, which had been prepared under his eye by Mr. Adolph Spiegel for experiments on the products of decomposition of that body. It was this specimen in which an analysis proved the presence of 0·0236 per cent. of potassium. So far, therefore, from confirming Dr. Thudichum's statement, this specimen was found by me to contain less than one-thirtieth of the amount of potassium which Dr. Thudichum asserts to be present in protagon.

That the first crystallizations of a proximate principle of the brain, such as protagon, should contain a trace of potash-salts is what would naturally have been anticipated from a knowledge of the nature of the soluble salts of the brain, and, therefore, to argue against the individuality of protagon, because of the presence of 0·0236 per cent. of potassium in a second crystallization-product, appears to be entirely fallacious.

April 7, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Minute Structure of the Lung of the Newt with especial reference to its Nervous Apparatus." By WILLIAM STIRLING, M.D., Sc.D., Regius Professor of the Institutes of Medicine (Physiology) in the University of Aberdeen. Communicated by Professor HUXLEY, Sec. R.S. Received March 19, 1881.

(Abstract.)

The lungs of the newt and triton are essentially simple sacs without any septa projecting into their interior, so that they are remarkably well suited for microscopic examination. They are covered externally by a layer of endothelium, but there are no stomata to be found between the endothelial cells. Under this is a small quantity of areolar tissue containing a plexus of yellow elastic fibres, with the long axis of the meshes arranged in the long axis of the lung-sac. Under this is a layer of non-striped muscular fibres, which forms a complete investment for the lung. These muscular fibres are disposed circularly. They present the same structure as similar cells in the mesentery of the newt. Each cell contains an intra-nuclear and intra-cellular plexus of fibrils.

The arrangement and distribution of the blood-vessels is then described. The pulmonary artery runs along one side of the lung and the pulmonary vein runs on the opposite side. The trunk of the pulmonary vein lies quite superficially, *i.e.*, next the peritoneal surface. It is covered only by the serous investment of the lung, so that it lies superficial to, *i.e.*, outside, the muscular layer. The pulmonary artery lies deeper, below the muscular coat. The capillaries are then described. No capillaries exist internal to the line of distribution of the pulmonary vein, but capillaries are found over, *i.e.*, internal to, the line of distribution of the pulmonary artery. The epithelium is then described. Ciliated epithelium is found along the course of the pulmonary vein and at the origin of its chief branches, but the other parts of the lung are covered by a single layer of squamous epithe-

lium. The ciliated epithelium is directly continuous with that lining the short trachea.

The *nerves* of the lung are very numerous and are branches of the vagus. They enter the lung in three or four main strands at its base. These strands are of unequal thickness, *i.e.*, a varying number of nerve-fibres enters into their composition. At once they proceed towards the pulmonary *vein*, which they follow very closely in their distribution. They form a plexus along the course of the vein, which is readily revealed with the aid of gold chloride. Only a few non-medullated nerve-fibres pass on to the pulmonary artery. The nerve-strands lie outside the muscular coat, and as they pass onwards in the pulmonary walls they give off branches right and left. A large number of multipolar nerve-cells exists in the course of the nerve-strands, and they are especially numerous where a branch is given off. More than twenty medullated nerve-fibres and a considerably larger number of non-medullated fibres enter the lung. The nerve-strands in their course along the pulmonary vein lie in spaces lined by squamous epithelium.

The branches of the nerve-strands lie outside the muscular coat. The axial cylinders split up into fibrils, many of which divide dichotomously, and afterwards unite to form a wide meshed *primary nerve plexus* external to the muscular coat. From this branches are given off which form a much finer *secondary plexus*, which gives off very fine branches which run towards the muscular fibres in which they seem to terminate. It cannot, however, be maintained that all the nerve-fibres which enter the lung terminate in the muscular coat. The majority of the non-medullated nerve-fibres distributed along the course of the pulmonary artery seem destined for the muscular tissue in its walls. The author has found a plexus of nerve-fibres in the adventitia and another in the muscular coat of the pulmonary artery. Some of the nerve-fibres must, undoubtedly, have other relations than those indicated above.

The author points out that, as the lungs are developed from the alimentary canal, it is to be expected that structures, the exact homologues and representatives of those occurring in the wall of the alimentary canal, may be expected to occur in the lung. He suggests therefore, that the non-striped muscle in the wall of the lung of the newt, and the tracheal and bronchial muscles in the mammalian lung, are the representatives of one or more of the muscular tissues of the alimentary canal; and, as each of these muscular tissues has a nerve plexus in relation with it, a similar condition may be expected to occur in the lung. The plexus in the wall of the lung is comparable to Auerbach's or Meissner's plexus, or perhaps to both.

II. "On an Electrodynamic Balance." By H. HELMHOLTZ, For. Mem. R.S., Professor of Physics in the University of Berlin. Received April 7, 1881.

In order to avoid the disturbances produced by the variations of direction and intensity of terrestrial magnetism in measuring the intensity of galvanic currents by their electromagnetic effects, I have tried to construct an electrodynamic balance. I have suspended at the ends of the lever of a smaller chemical balance, instead of the scales, two coils of copper wire, their height being equal to the diameter of the cylinder around which the wire is coiled up. Their axis is vertical, and they are suspended in such a manner that they cannot turn around this axis. Two larger spirals of the same height, but of greater radius, are placed into a fixed position, borne by a horizontal metallic rod, the middle of which is fixed on the column bearing the balance. The connexions of the wires are arranged in such a way that one of the movable coils is attracted by the fixed coil, the other is repelled. Both the fixed coils are placed a little higher than the movable coils. The attracted coil rises, the other sinks down as soon as a current passes through the circuit.

There are two difficulties to be overcome in the construction of such a balance. At first, the current must be introduced into the movable spirals without diminishing their mobility, and without introducing places of contact of too small a pressure, which would make the resistance variable. I have succeeded to do this in a very satisfactory manner by using a kind of very thin sheet-brass, used for playthings of children, called in German "Rauschgold" (tinsel), because it looks like gold, and makes a crackling noise when it is moved. Strips of this, about 30 centims. long and 6 or 7 millims. broad, are very flexible, and show no signs of internal friction, their resistance to electric currents is very moderate, and they are not easily heated even by strong currents, because they have a relatively large surface in contact with air. I have connected each of the movable spirals with the other wires conducting the current by two such strips hanging loosely down from four pieces of brass fixed at the upper parts of the case of the balance. I may be allowed to remark, that strips of the same kind, and of greater length, are very useful to demonstrate the action of a magnet on a movable current. If you suspend the strip so that it hangs down in a curve, it is attracted, repelled, even raised against gravity, or coiled up around the magnet with great rapidity, in a very striking way.

The second difficulty is to bring the coils into such a position that neither the stability nor the sensibility of the balance is impaired. In order to do this, it is necessary that the intensity of the electrodynamic force does not vary sensibly during the usual small oscillations of the balance. Now the force is zero if the middle of the movable coil is at the same height as that of the fixed coil. The force is again zero if the distance of the two coils becomes infinite. Between those two zero points there exists a maximum value of the force, which corresponds nearly to that situation where the upper surface of one of the coils is at the same height as the under surface of the other. Between the central position of the two coils and this position of maximum force, therefore, the differential coefficient of the force related to increasing distance of the centres is positive, and turns into negative when we pass the position of the maximum. This differential coefficient becomes again zero at an infinite distance. Therefore, between the position of maximum force and infinite distance there must be a distance where the differential coefficient of the force has itself a negative maximum, and the second differential coefficient, therefore, is zero. This is the position which must be given to the coils. As always the distance of one pair of the coils is diminished as much as that of the other pair is increased, the variation of the force depends only on the second differential coefficient. If this is positive, the electric current produces unstable equilibrium; if it is negative, the stability of the equilibrium is increased; that is to say, the balance becomes less sensitive than it is without current. If the coils are brought into the right distance, neither the sensibility nor the stability of the balance is altered, and by this circumstance itself the right position can be found out.

If the instrument is well adjusted, you can determine the weight which balances the electrodynamic force with errors not exceeding one milligramme. As the force exerted by the current is proportional to the square of its intensity, you determine the intensity of a current, which is counterbalanced by one gramme with an accuracy of $\frac{1}{2000}$, and the force which opposes the electrodynamic force and measures it is gravity alone, and therefore not subject to any variations, like those of terrestrial magnetism, or like the elasticity of a twisted wire on which one of the coils is suspended.

The observations of the electrochemical equivalent of the current corresponding to one gramme of weight, performed by different observers during the course of last year, have given a very satisfactory agreement.

III. "On the Internal Forces of Magnetized and Dielectrically Polarized Bodies." By Professor H. HELMHOLTZ, For. Mem. R.S. (Oral Statement at the request of the PRESIDENT.)

The Society then adjourned over the Easter Recess to Thursday, April 28th.

April 28, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "The Influence of Stress and Strain on the Action of Physical Forces." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received April 5, 1881.

(Abstract.)

PART I.—ELASTICITY.

"*Young's Modulus.*"

The values of "Young's modulus" were determined for several metals by a method devised by Sir W. Thomson. According to this method wires of the same material and diameter are suspended in pairs about an inch apart from each other, and are attached by one extremity of each to the same support; the other extremities being fastened in the one case to a scale-pan, and in the other to the centre of a bar of wood or metal carrying constant equal weights at each end: the latter wire is provided with a scale and the former with an index of some sort, which being level with and close to the scale, serves to measure any alteration of length produced by weights placed in the pan.

In these particular experiments the wires were about thirty feet in length; a scale divided into half-millimetres and a vernier reading to $\frac{1}{20}$th of a millimetre attached respectively to the two wires, served to measure the temporary alterations of length produced by loading or unloading. The vernier was forked so that, though capable of free up and down motion, it could not readily be dislodged sideways, and by using a compound microscope an alteration $= \frac{1}{100}$th of a millimetre in the length of the wire could be estimated.

A large number of experiments with different loads were made, and after a great many unsuccessful attempts to account for certain discrepancies which could not be explained away as errors of observation, the following facts were elicited:—

(1.) After a wire has suffered permanent extension, the temporary elongation which can be produced by any load becomes less as the interval between the period of permanent extension and that of applying the load becomes greater.

(2.) This increase of elasticity is greater in proportion for large loads than for small ones.

(3.) The increase of elasticity takes place whether the wire be allowed to remain loaded or unloaded between the period of permanent extension and that of the testing for the elasticity.

(4.) The *rate of increase* of elasticity varies considerably with different metals; with some the maximum elasticity is apparently attained in a few minutes, and with others not till some days have elapsed, iron and steel being in this last respect very remarkable.

(5.) The elasticity can also be increased by heavily loading and unloading several times, the rate of increase diminishing with each loading and unloading.

(6.) A departure from "Hooke's law" more or less decided, always attends recent permanent extension even when the weights employed to test the elasticity do not exceed one-tenth of the breaking weight.

(7.) This departure is diminished very noticeably in the case of iron, and much less so in the case of other metals, by allowing the wire to rest for some time either loaded or unloaded; it is also diminished by repeated loading and unloading.

With aluminium and zinc both the maximum temporary increase of length caused by putting on weight, and the recovery on the removal of the load are attained only after several hours, if the weights employed be not very small.

With tin and lead the loads employed were so small, in order to avoid permanent set, that the values of "Young's modulus" obtained for these metals cannot be relied on within 2 per cent.

A small though decided departure from "Hooke's law" was found in all cases, and the experiments abundantly proved that this law can only hold good practically for much smaller loads than are usually employed in determining the value of "Young's modulus" from the method of static extension.

A discussion of Wertheim's* experiments on elasticity will be found in the paper, and it is there pointed out that a principal cause of the differences of the values of "Young's modulus," obtained by him from the method of static extension, and from longitudinal or transverse vibrations, is to be found in his manner of experimenting according to the first of these methods.

The effect of permanent extension on the value of "Young's modulus," was tried according to the direct method for iron and copper, and indirectly for most of the metals.

From both the direct and indirect methods results were obtained which showed :—

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* "Ann. de Chem. et Phys.," tom. xii, 1844.

(1.) That, in all metals, provided the wire has not been kept heavily loaded for some time before testing, permanent extension produces decrease of elasticity, if the strain be not carried beyond a certain limit.

(2.) That, if the extension be carried beyond the above-mentioned limit, further permanent increase of length causes increase of elasticity.

(3.) That, in the case of iron, heavy loading for some time so increases the elasticity that, even when the extension would have caused diminution of elasticity without such continued loading, the latter will, if sufficient time be allowed, change this diminution into an increase; in the case of copper this is not so.

It was also observed that, with iron which has been very heavily loaded for some time, the ratio of the temporary elongation to the load producing it becomes less as the load employed becomes greater, until a certain limit, depending upon the extent of the previous heavy loading, has been reached; whereas, with the other metals, and with iron which has suffered permanent extension without allowing the load producing the extension to remain for any appreciable time on the wire, the elongation increases in a greater proportion than the load.

The behaviour of iron in this respect, as well as the fact that this metal does not, until many hours have elapsed, attain its maximum elasticity, after having undergone permanent elongation, is probably to be attributed to the great coercive force of the metal.

To a similar cause may also be assigned the well-known increase of portative power of a magnet which can be produced by gradual loading; and the great difference between the tenacity of iron when loaded by slow degrees, and when loaded quickly.*

One of the above-mentioned indirect methods of determining the effect of permanent extension on the elasticity consisted in combining torsion with traction. The wire to be examined, some 95 centims. in length, passed through a small hole in a stout table, and was clamped at its upper extremity into a brass block, the latter resting on the table, and being sufficiently secured by a heavy weight placed on the top of it. Near the lower extremity, which was looped to receive a scale-pan, was clamped a second small brass block, to one end of which was attached a light mirror; the latter used in connexion with a scale and lamp enabled the observer to detect very small changes of torsion. The wire having previously received a certain amount of permanent torsion was loaded and unloaded several times with different weights, and the amount of temporary twist or untwist produced thereby determined.

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* See J. T. Bottomley's experiments, "Proc. Roy. Soc.," vol. 29, p. 221.

The effect of suddenly chilling steel heated to a high temperature was found to be similar to that of excessive permanent extension of iron; and in general, it was concluded to be highly probable that whether the distance between the molecules be increased by mechanical strain or by the strain caused by sudden cooling, the elasticity in the direction of the line of separation of the molecules diminishes to a minimum as the separation increases, and then begins to increase.

Moreover, it would appear from the results which will be described in the other parts of this paper, that most, if not all, the physical properties of a substance are affected in a similar manner by stress and strain; whether these latter be the result of mechanical or of *any physical* agency.

Torsional Rigidity.

The torsional rigidity of the wires was determined by the method of vibrations. The vibrators were similar to those employed by Sir W. Thomson in his experiments on the rigidity and viscosity of metals,* namely, thin cylinders of sheet brass, supported by a thin, flat, rectangular bar. The wire to be tested passed perpendicularly through a hole in the middle of the bar, and was there soldered; the other end of the wire was then soldered into a stout iron bar, firmly held in a vice attached to a rigid support.

Great difficulty was experienced in making good observations of the rigidity of zinc, tin, and lead, in consequence of their great viscosity.

Several experiments were made to test the effect of permanent torsion and permanent extension on the modulus of rigidity.

From these experiments was concluded:—

(1.) That the loss of rigidity produced by twisting or stretching a wire beyond the limits of elasticity is partly diminished by rest.

(2.) That the loss is more sensible with large arcs of vibration than with small ones.

(3.) That the influence of rest is more apparent in the case of large vibrations than in that of small ones.

(4.) That continual vibrating through large arcs has a similar effect on the rigidity to that produced on the longitudinal elasticity by heavily loading and unloading. And—

(5.) That in the case of hard steel the effect of vibrating through a large arc for several minutes makes temporarily the rigidity as determined from such vibrations greater than that determined from smaller vibrations.

From the values of the torsional rigidity and "Young's modulus" were calculated the ratios of lateral contraction to elongation for the different metals, the formula used for this purpose being $\sigma=\frac{e}{2r}-1$, in which e=" Young's modulus," r=rigidity, both in grams per square centimetre, and σ=the required ratio.

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* "Proc. Roy. Soc.," vol. 14, p. 289.

In the following table are given the values of e, r, and σ for most of the annealed metals:—

Name of metal.	" Young's modulus." e.	Torsional rigidity. r.	Ratio of lateral contraction to elongation. σ.
Iron..................	1981×10^6	$773 \cdot 1 \times 10^6$	·281
Platinum	1490	692·7	·076
German silver.........	1335	493·7	·354
Copper	1160	440·6	·315
Platinum-silver	1051	369·9	·420
Zinc..................	767	338·4	·133
Silver	742	271·8	·367
Aluminium	673	265·2	·269
Tin	277	120·9	·145
Lead.......	167	74·0	·136

The mean value of σ for the *different* substances employed in the annealed condition =·2515, a number closely according with that assigned by Poisson from mathematical considerations, as the value of σ for *each*.

The metals copper, platinum, aluminium, silver, and platinum-silver were obtained from Messrs. Johnson, Matthey, and Co., as chemically pure, and the zinc, lead, and tin wires as being as pure as could be got from the ordinary process of distillation.

Experiments on the permanent alteration of density which can be produced by longitudinal traction proved, as the investigations of Wertheim and Thomson have already shown, that such alteration is very slight. In these particular experiments no change amounting to $\frac{1}{2}$ per cent. was detected, though the wires were strained to breaking.

Certain calculations were made to verify Wertheim's formula $e \times \alpha^7$=a constant, where e=" Young's modulus," and α=the mean distance between one molecule and another, and it was found that this formula was approximately true. Moreover, the products of r and α^7 were calculated and found to be approximately constant for the different metals.

The influence of an electric current and of magnetism on the torsional rigidity of metals was also investigated, and the following results arrived at:—

(1.) The torsional rigidity of copper and iron is temporarily decreased by the passage of a powerful electric current, but is very little, if at all appreciably, altered by currents of moderate intensity.

(2.) The torsional rigidity of iron is temporarily diminished to a small but perceptible extent by a high magnetising force.

(3.) The effects mentioned in (1) and (2) are independent of any changes produced by the current in the temperature of the wire.

Finally, certain critical points are alluded to, there being at least two such for each metal, at which sudden changes take place in the ratio of the permanent extension produced by any load and the load itself.

The existence of the first of these critical points seems to prove beyond a doubt that, in all well-annealed metals, there is a true limit of elasticity, which is intimately connected with the elasticity of the substance, and it will appear from the investigations made in the other parts of this paper, that changes more or less profound take place in most, if not all, of the physical properties of the substance at these points.

II. "Lucifer: a Study in Morphology." By W. K. Brooks, Associate in Biology and Director of the Chesapeake Zoological Laboratory of the Johns Hopkins University, Baltimore, Md., U.S.A. Communicated by Professor Huxley, Sec. R.S. Received April 6, 1881.

(Abstract.)

Our knowledge of the life-history of this extremely interesting genus is very scanty, and the only published observations are contained in a short paper, without illustrations, in "Proc. Roy. Soc.," vol. 24, p. 132, by Willemöes-Suhm.

The death of this naturalist, only a few months after his paper was written, put an end to his studies, which he had expressed a hope of finishing, and I therefore take pleasure in stating that I have been so fortunate as to procure the eggs of *Lucifer*, and to trace every stage of the metamorphosis, from the time the larva leaves the egg up to the mature male, by the actual moulting of isolated specimens in captivity; and I have also been able to add a few observations upon its embryology.

The early stages of development are extremely interesting, since the segmentation is quite different from that of any arthropod egg which has been described. The egg undergoes total regular segmentation; there is a true central segmentation cavity, and during the early stages there is nothing to represent the yolk-pyramids of the ordinary crustacean egg. When segmentation is somewhat advanced, the granular matter becomes restricted to one of the spherules; this then pushes into the segmentation cavity, and appears to correspond to a single yolk-pyramid. Its outer end splits off as a blastoderm cell,

and its inner granular end, which is now situated in the segmentation cavity, soon divides up into a number of food-yolk spherules.

The primitive digestive cavity is next formed by an invagination, at the point where the yolk-pyramid has been pushed in.

The larva leaves the egg as a *Nauplius*, which, after two moults, is changed into a *Protozoœa*, with nauplius-like antennules and antennæ; mandibles like those of the adult; two pairs of maxillæ; two pairs of maxillipeds, and a long hind-body divided at its anterior end into four zoonites.

After a number of moults it becomes changed into Dana's *Erichthina demissa*, which Willemöes-Suhm has shown to be a *Lucifer* larva.

The *Erichthina* gradually changes by three moults into Dana's *Serbatina armata*, which seems to be the same as Willemöes-Suhm's *Amphion* stage.

After a number of moults it becomes a *Mastigopus*, and then both sexes gradually assume the adult female form, the characteristics of the adult male appearing last.

The first four sections of the paper describe the structure, embryology, and metamorphosis of *Lucifer.* Among the more important points are the total absence at all stages of the fifth thoracic zoonite and its appendages; the origin of the metastoma as a pair of buds homologous with the other appendages, and the absence of larval stages with the distinctive features of the *Elaphocaris* and *Acanthozoœa* larvæ of *Sergestes.*

The fifth section gives an account of the metamorphosis of another Sergestid, which there is reason to believe to be *Acetes*, although it was not reared to the adult form. At the *Elaphocaris* and *Acanthozoœa* stages this form is intermediate between the highly modified larva of *Sergestes* and the simple larva of *Lucifer.*

The sixth section discusses the relation between the larvæ of *Lucifer*, *Acetes*, *Sergestes*, *Penœus*, and *Euphausia*, and the significance of the deapod *Zoœa* and crustacean *Nauplius.*

As one result of the comparison, I attempt to show that, while the life-history of *Lucifer*, *Acetes*, *Sergestes*, and *Penacus*, tends to confirm Claus' view that the *Zoœa* is a secondary modification of the more primitive *Protozoœa*, a comparison of these forms with *Euphausia* shows that this view is not without difficulties, since these genera differ from each other in the very point in question; the manner in which the zoonites and appendages of the thorax make their appearance.

The seventh section discusses the phenomena of serial homology and bilateral symmetry in the Crustacea, and is an attempt to show that these resemblances cannot be explained by heredity from the parts of an unspecialised form, since the facts of embryology do not support the view that the remote ancestor of the Crustacea was a long series of independent zoonites or metameres. Even if this view were supported by facts, the embryology of *Lucifer* compels us to recognise a recent and persistent bond of homology between its appendages.

The next section is an attempt to show that, while we are unable to believe that serially homologous organs, in all cases, owe their resemblance to heredity from the unspecialised organs of a remote ancestor, they may still be due to heredity; but to heredity from the same part of the developing egg, rather than from a remote ancestor.

The last section is an attempt to show that, by making a slight change in Darwin's "Pangenesis Hypothesis," it can be so simplified, that the germinules which it demands may be few in numbers, simple in their properties, and not infinitely small. In its modified form the hypothesis accounts for the phenomena of serial homology and symmetry, and it removes some of the most serious difficulties of the theory of natural selection, especially by showing why variations appear when they are needed.

Presents, March 24, 1881.

Transactions.

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"Influence of Voltaic Currents on the Diffusion of Liquids."[*] By G. Gore, LL.D., F.R.S. Received December 1, 1880. Read December 16.

1. *General Statement of the Phenomena.*

In a recent preliminary statement (see "Proc. Roy. Soc.," vol. 30, p. 322) I have mentioned that "when an electric current was passed between the surfaces of mutual contact of certain aqueous solutions of different specific gravities, the boundary surfaces of contact of the two liquids became indefinite where the current passed from the lighter into the heavier solution, and became sharply defined where the current left the heavier and re-entered the lighter liquid; and that on reversing the direction of the current several times in succession after suitable intervals of time, these effects were reversed with each such change. Also, in various cases in which the contiguous boundary layers of the two liquids had become mixed, the liquids separated, and the line of separation of the two solutions became by the influence of the electric current as perfect as that between strata of oil and water lying upon each other."

These and other phenomena which accompany them form the subject of the present communication.

The chief additional phenomena observed have been—1st. The production of definite lines, not only where the current passed from the heavier into the lighter solution, but also (in certain cases) at the surface where it passed from the lighter to the heavier one; 2nd. The production in some cases of two or three separate lines at the former situation, and less frequently also at the latter one; and, 3rd. An apparent movement of the mass of the heavier solution, usually in the direction of the electric current, but in certain exceptional cases in a reverse direction. In all cases there occurred in addition the more or less influencing circumstance, rise of temperature (by conduction, resistance, &c.), with its usual effects.

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[*] This research arose out of one on "The Phenomena of the Capillary Electroscope." See "Proc. Roy. Soc.," vol. 31, p. 295.

The earliest allied experiment known to me is one by Faraday (Exp. Res. 494), who passed an electric current from forty cells, four inches square, by means of platinum plate electrodes, through a strong solution of magnesic sulphate into a layer of distilled water, half an inch deep and about seven square inches in area, lying upon it. In less than one minute magnesia appeared at the boundary surface of the two liquids, forming a layer one-eighth of an inch deep in the water immediately *upon* the magnesic solution. The water above was quite clear. There was no alkali at the negative platinum plate, but plenty of acid at the positive one. This experiment was repeatedly verified. Daniell subsequently in a research on the "Electrolysis of Secondary Compounds" ("Phil. Trans.," 1840, pp. 209–224), passed an upward current through solutions of the nitrates of silver, mercury, and lead, and the sulphates of palladium, copper, iron, and magnesium into a dilute one of caustic potash, separated from them by a thin horizontal diaphragm of bladder. Oxygen was determined to the upper, and the respective metals to the lower, surface of the diaphragm, and deposits of metal, more or less oxidised, were formed upon the latter surface, the oxidation being more complete the more oxidisable the metal; with the magnesium salt oxide alone was obtained.

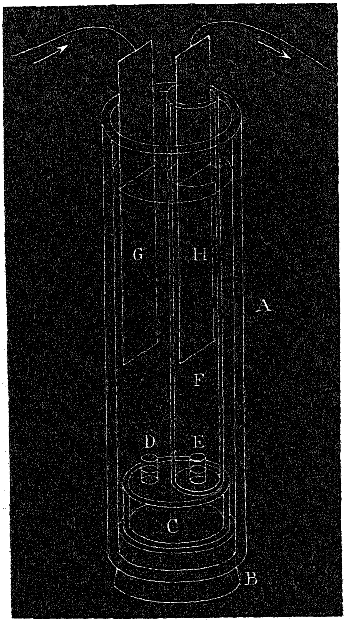
2. *The Influence of Kind and Strength of the Liquids.*

In order to obtain a general basis of facts, I made a number of experiments with the following apparatus:—

A is a glass cylinder, 20 centims. high and 6 or 7 centims. diameter, open at both ends; B is a vulcanised india-rubber bung, tightly fitting the cylinder; C is a glass cup (of nearly equal diameter to that of the inside of the cylinder), with a perfectly flat and watertight lid of gutta-percha, secured by means of varnish; D and E are open tubes of glass, about 15 or 18 millims. high and 8 or 10 millims. internal diameter, flat at their ends, and fitting tightly in the lid of the cup; F is an open glass tube, about 17 centims. high and 26 millims. diameter, perfectly flat at its lower end; G and H are large electrodes of sheet platinum.

The apparatus whilst being used should be situated in a strong light. The cup is first filled with a heavy and strongly coloured electrolyte to about half the height of the tubes D and E, care being taken to exclude all bubbles of air, and the lid of the cup is coated with grease over that portion upon which the end of the tube F is to stand; the cup is then placed upon the bung B, and the latter inserted tightly in the cylinder, and the cylinder fixed steady. Two corks (secured upon the ends of stout platinum wires, about 20 centims. long) are now inserted in the ends of the tubes D and E, and the cylinder A nearly filled with the lighter and colourless electrolyte. The electrode of the tube F is now fixed in position by means of a cross-bar of cork. The tube (enclosing one of the platinum wires) is next simultaneously immersed, together with the electrode G, in the liquid, and its lower end very slowly and carefully placed so as to stand perfectly flat and watertight upon the lid of the cup. Both the upper end of the tube F and of the electrode G are now fixed tightly in the cylinder by means of pieces of cork. The cork from the tube E is then withdrawn with extreme slowness, so as not to destroy the definite meniscus, and then the one in D similarly removed. The current from five or ten (or more) Grove's elements is now at once passed from G to H (before the liquids in D and E have time to mix), and the effects upon the

FIG. 1.



contact surfaces of the two liquids (and especially upon the portions of liquid immediately above them) continually observed and noted.

To those of the lower liquids which were colourless, strong colour was imparted by addition beforehand of solution of litmus. Although purity of the liquids does not appear to materially affect the phenomena under examination, distilled water, pure acids, and usually salts of a high degree of purity were employed in all cases. In all the first six experiments of the following series a current from a single row of five Grove's elements, each of one pint capacity, was used; and the apparatus already described was employed in all those to No. 36.

Experiment 1.—Meniscus tubes 12 millims. high and 3·5 millims. bore. Lower liquid, a saturated solution of cupric chloride of a green colour; upper one, dilute sulphuric acid, 1 in 10. After passing the current during five minutes, the cupric solution in the tube under anode was deep green, and its meniscus indefinite; and under cathode blue, with meniscus definite. Reversed current repeatedly and obtained similar effects each time after about ten or fifteen minutes. *Remarks.*—The blue colour at the positive end of the cupric solution was probably due to the accumulation of basic chloride by the influence of the current.

Exp. 2.—Same tubes. Lower liquid, a saturated aqueous solution of cupric sulphate; upper one, same as in last experiment. Free conduction. Positive meniscus definite and negative one indefinite.* By reversing the current the previously definite one thickened out and rapidly became lower in position, whilst the other one gradually became definite and rose in position, as if the mass of the liquid moved in the same direction as the current.

Exp. 3.—Same tubes. Lower liquid same as last; upper one, 1 volume of hydrochloric acid and 6 of water. Conduction free. No green colour at negative meniscus below anode. (Found by a separate experiment that the addition of hydrochloric acid to a solution of cupric sulphate did not make the latter green.) Positive meniscus was definite, negative one indefinite and diffused. The anode became coated with solid hydrate of chlorine, which fell off and decomposed with evolution of gas on stopping the current.

Exp. 4.—Meniscus tubes 15 millims. long and 4 millims. bore, were now used in all experiments to No. 12. Lower liquid a saturated solution of cupric chloride; upper one a saturated solution of potassic sulphate. Conduction moderate. Positive meniscus definite, and soon a second and parallel line appeared beneath it, the intervening stratum being weak in colour; negative meniscus diffuse. Reversed the current, first an upper, and then a second and lower line appeared under the cathode; the other meniscus became diffuse and indefinite.

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* By positive meniscus I mean the one under the cathode, and by negative that under the anode.

Exp. 5.—Lower liquid, 1½ ozs. of potassic bichromate dissolved in 12 ozs. water; upper one a half saturated solution of sodic carbonate. Conduction moderate. The bulk of the lower liquid appeared to move in a direction opposite to that of the current, and its level became 8 millims. lower in the tube under the cathode than in that under anode. A double meniscus line formed in the positive tube (*i.e.*, under cathode). Gas was evolved at the positive meniscus, probably carbonic anhydride. Positive meniscus became ultimately 12 millims. lower than negative one. On stopping the current the positive meniscus rose and the negative one fell immediately.

Exp. 6.—Lower liquid, a saturated solution of cupric sulphate, upper one dilute nitric acid 1 in 12. Copious conduction. Blue liquid rose in tube under cathode, and sank in that under anode. Positive meniscus very definite, negative one diffuse.

Exp. 7.—Now employed a single series of six Grove's elements in all the experiments to No. 34. Lower liquid a saturated solution of mercuric nitrate (not coloured); upper one, dilute nitric acid 1 in 12. Free conduction. A layer of white powder, 25 millims. thick, soon accumulated at the positive end of the lower liquid. Reversal of current quickly caused the salt to disappear, and to reappear at the opposite (now the positive) meniscus. *Remarks.*—The white powder was probably salt accumulated by the influence of the current.

Exp. 8.—Lower liquid a nearly saturated solution of yellow potassic chromate; upper one same as in last experiment. A thin layer of solid red salt set free at each meniscus by chemical action. Now passed the current. Salt at positive meniscus dissolved quickly. Positive meniscus descended slowly and negative one ascended, as if the lower solution moved in an opposite direction to that of the current.

Exp. 9.—Lower liquid a saturated solution of cupric nitrate; upper one same as in last experiment. Copious conduction. The definite line commenced to be visible at positive meniscus in two minutes, and soon became of a deeper blue colour than the layer of solution immediately below it, as if the blue colour ascended and caused the colour to become concentrated as it progressed.

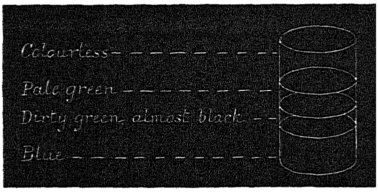
Exp. 10.—Lower liquid a saturated solution of cupric chloride; upper one same as in last experiment. Copious conduction. Definite line of blue colour soon formed at positive meniscus. The solution below negative meniscus being green. Reversal of current reversed these effects. *Remarks.*—The line of blue colour at the positive meniscus may have been due to electrolytic accumulation of basic chloride, or to electric diffusion of nitric acid. Dilute nitric acid does produce such a change of colour in a solution of cupric chloride.

Exp. 11.—Tubes 18 millims. long and 5 millims. bore. Lo[illegible] liquid a nearly saturated solution of cupric nitrate; upper one dilute nitric acid 1 in 10. Very free conduction. In two minutes the negative meniscus became diffused, and the positive one a distinct line; the solution was of a lighter colour 3·0 millims. below the latter meniscus than lower down. Several reversals of the current reversed these effects each time in a few minutes, the period requisite increasing each time.

Exp. 12.—Same tubes as in Experiments 5 to 11. Lower liquid a saturated solution of cupric nitrate; upper one dilute hydrochloric acid 1 volume to 8 of water. Copious conduction. Coloured liquid below anode seemed to sink. A definite line appeared at positive meniscus in one and a half minute. By reversal of current that line became indefinite almost at once, and a definite one appeared at the other surface of contact in two and a half minutes. The mass of lower solution appeared to move in the direction of the current.

Exp. 13.—Tubes 15 millims. long and 7·5 millims. bore. Lower liquid same as in last experiment; upper one, 2 ozs. potassic bromide dissolved in 12 ozs. water. Each contact surface of the lower liquid became green previous to passage of current. Conduction moderate. Green colour extended downwards at surface below anode, and did not decrease at the other meniscus. Definite lines were produced at both meniscuses, the negative meniscus also became bluish. Reversed current, a definite line was produced in an extremely dilute part of lower solution below cathode; the other appearances were also reversed.

FIG. 2.



Exp. 14.—Lower liquid the same as in the last experiment; upper one dilute nitric acid 1 in 10. Two definite horizontal lines formed in the lower liquid below cathode in about one minute, about 1·75 millim. asunder, with a pale yellow stratum of liquid beneath them, and soon merged into each other. The other meniscus sank and became diffuse.

Exp. 15.—Lower liquid a strong green solution of cupric bromide; upper one same as in last experiment. Upper surfaces of lower solution were convex and became blue without the aid of the current. Copious conduction. Blueness of surface disappeared from negative meniscus and increased to positive one, and became deeper in colour to 1·75 millim. thick. Definite line at surface below cathode, and diffused meniscus below anode. Negative meniscus became strongly convex and cloudy, the other one was blue, clear, and slightly concave; its blue portion became nearly 12 millims. thick. Reversal of current was soon followed by reversal of these effects. The total bulk of the lower liquid increased during the experiment.

Exp. 16.—Lower liquid a saturated solution of potassic cyanide, coloured blue by litmus; upper one, 2 ozs. potassic chloride dissolved in 12 ozs. of water. Conduction copious. Definite line produced in two minutes at about 2 millims. above the blue solution below cathode, and the blue colour soon ascended to it. Positive meniscus became definite and greenish. The mass of lower liquid appeared to move in a direction opposite to the current.

Exp. 17.—Lower liquid same as in last experiment; upper one, 4 ozs. of a saturated solution of sodic carbonate and 8 ozs. of water. Weak conduction. The line at negative meniscus was more definite than that at positive one. After reversal of current this effect was also reversed. The line began to form at about 2 millims. above the blue liquid under anode. The blue liquid appeared to move in a direction contrary to that of the current.

Exp. 18.—Tubes 18 millims. long and 7·5 millims. bore. Lower liquid as in last experiment; upper one, 1 oz. of acid sulphate of sodium in 12 ozs. of water. Free conduction. A line appeared at once in very dilute part of cyanide solution below anode, and on reversing current it appeared immediately in the other tube. Soon also solid salt formed at the positive meniscus (probably potassic sulphate), as if much downward electric diffusion of acid occurred there, the colour also became weaker; no salt formed at the very definite negative meniscus. By reversing the current a dilute space was produced between the deep blue coloured cyanide below and the light blue above near the line of negative meniscus. The mass of lower liquid appeared to move in the direction of the current, contrary to what usually occurs with a solution of potassic cyanide. The total bulk of the blue liquid increased.

Exp. 19.—A one-fourth saturated solution of acid sulphate of sodium beneath, and dilute nitric acid 1 to 10 above. Copious conduction. In three minutes a line commenced to appear 1·75 millim. above positive meniscus in pale red portion. Liquid appeared to move in the direction of the current.

Exp. 20.—Same solution of coloured potassic cyanide below as in Experiment No. 18, and one of 1½ oz. of potassic ferrocyanide in 12 ozs. of water above. Free conduction. Definite concave line in half a minute in tube below anode. Stopped current, the line became at once flat. Reversed current, a curved line appeared in colourless portion of liquid of tube below the anode in 1½ minute, 1·75 millim. above visible blue portion, and sank down into the blue in half a minute. Blue colour in tube below cathode rose, and became weaker than in the other. Reversed current again, line in tube below anode produced immediately.

Exp. 21.—Lower liquid a saturated solution of chloride of cobalt, upper one dilute nitric acid 1 to 10. Copious conduction. Two lines soon formed about 2 millims. apart in the liquid of tube below cathode, they merged and formed one concave line. The lower liquid appeared to move in the direction of the current. By reversal of current the line appeared in the tube under the cathode, and the coloured liquid in the tube under anode became diffuse. Previous to the line appearing, the surface of lower liquid under the cathode was convex; when the line first appeared that surface became straight, and when the line was fully developed it was concave. The liquid again appeared to move in the same direction as the current.

Exp. 22.—A saturated solution of yellow potassic chromate below, and one of 3 ozs. of sodic sulphate in 12 ozs. water above. Conduction free. A line appeared in one minute, in liquid of tube below anode, at the verge of dilute part; a second line also appeared but did not merge into the first; a third line also came, but did not merge into the next one. The solution in the tube below anode diffused upwards, and that in the other concentrated downwards, so that the mass appeared to move in a direction opposite to the current. Reversed the current, similar effects occurred.

Exp. 23.—Lower liquid a saturated solution of perchlorate of nickel; upper one dilute nitric acid 1 to 10. Conduction moderate. In two minutes the surface beneath cathode became perfectly flat and definite, and then concave. Soon also a second line appeared 5 millims. below the other, with increased colour of solution beneath. The liquid in the other tube became diffused in colour. Reversed current, similar effects occurred after about ten minutes.

Exp. 24.—Small tubes 16 millims. long and 10·5 millims. bore. Lower liquid 1 volume of sulphuric acid to 2 of water, coloured by litmus; upper one dilute sulphuric acid 1 in 20, not coloured. Conduction free. Positive meniscus became a little more definite. No other apparent effect.

Exp. 25.—A saturated solution of cupric nitrate beneath and dilute nitric acid above. Moderate conduction (battery weak). A definite line, concave and rising, developed in tube under cathode. Reversed current, similar effect but feebler.

Exp. 26.—Lower liquid a saturated solution of sodic carbonate, coloured by litmus; upper one dilute solution of the same salt, consisting of 1 volume of the saturated solution and 12 volumes of water. Feeble conduction. The liquid in tube below anode diffused, that in the other tube did not. Mass of lower liquid did not appear to move in direction of the current, but ascended by diffusion at negative meniscus. No production of a visible line.

Exp. 27.—Small tubes each 14 millims. high, one 5 millims. bore, and the other 15 millims. Lower liquid 1 volume of sulphuric acid to 2 of water, coloured; the upper one consisted of 1 volume of sulphuric acid and 20 of water. Battery strong, current from broad to narrow tube. In two minutes a line appeared in narrow tube about 2 millims. above red liquid; the liquid rose a little in the tube. *Remarks.*—Experiments Nos. 24, 26, and 27 show that it is difficult to obtain the definite line with liquids which differ only in strength.

Exp. 28.—Lower liquid a saturated solution of cupric nitrate; upper one, dilute sulphuric acid 1 in 8. Current from broad to narrow tube. Two lines developed in narrow tube 3·0 millims. asunder; the upper one concave and the lower one flat. No line produced in broad tube. Reversed current, lines in narrow tube disappeared, and the meniscus became diffused. Two lines developed slowly in broad tube 1·75 millim. apart, the upper one concave in weakest part of the red liquid; the lower one flat, and merged after a while into the other.

Exp. 29.—Lower liquid same as in last experiment; upper one dilute nitric acid, 1 volume of acid to 5 of water. Current from broad to narrow tube. Free conduction. A straight line developed in narrow tube in about forty-five seconds. Reversed current, a line commenced to appear in broad tube in about seven minutes, and was fully developed in about four minutes more. Reversed again. Straight line developed in narrow tube in about four minutes; it quickly became concave, and the liquid rose in that tube as if moving in the direction of the current.

Exp. 30.—Lower liquid a saturated solution of sodic carbonate, coloured by litmus; upper one dilute solution of the same salt, viz., 1 oz. of the saturated solution and 12 ozs. of water. Current from broad to narrow tube. Conduction very feeble, though battery was strong. No line produced in either tube. Liquid in narrow tube sank and became weaker in colour and diffused; that in the broad tube rose slightly and became less diffused. The mass of the liquid appeared to move in a direction opposite to that of the current.

Exp. 31.—Lower liquid a saturated solution of potassic cyanide, coloured by litmus; upper one, 1 oz. of the same salt in 12 ozs. water. Current from broad to narrow tube. Conduction feeble. An imperfectly developed line produced in narrow tube. The coloured liquid sank in the narrow tube as if moving opposite to the current. Liquid surface was less definite in the broad tube than in the narrow one.

Exp. 32.—Lower liquid, equal volumes of nitric acid and water, coloured; upper one, 1 volume of nitric acid and 10 of water. Both meniscuses convex. Current from broad to narrow tube. Free conduction. Imperfectly developed line in colourless portion of liquid in narrow tube in four minutes, 2 millims. above the red liquid. The red liquid rose in narrow tube as if moving in same direction as the current. *Remarks.*—Experiments Nos. 31 and 32 also show the difficulty of producing definite lines in liquids which differ only in degree of concentration.

Exp. 33.—Now employed a current from twelve Grove's cells arranged in single series, until Experiment No. 37. Small tubes 15 millims. high and 8 millims. bore. Lower liquid same as in last experiment; upper one, 1 volume of nitric acid and 5 of water. Very copious conduction. Deep curved line produced in tube beneath cathode, and three distinct lines in the other tube, the middle one of which merged itself into the upper one, and the two others remained. The lines were not as definite as those produced by two liquids of more widely differing chemical composition.

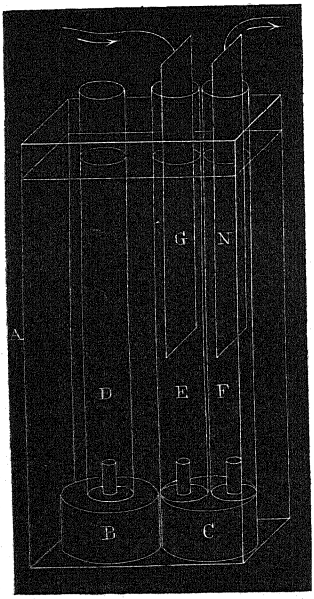
Exp. 34.—Small tubes 17 millims. high, 5·5 millims. bore. Liquids same as in last experiment. Copious conduction. A definite line produced in colourless part of liquid in tube below cathode 1·75 millim. above coloured portion, and a very faint one produced in the other tube. These lines disappeared during the stoppage of current, and reappeared whilst it was passing. They disappeared in about two or three seconds and reappeared in about six seconds, and were the most definite in the tube under cathode. This was verified several times.

3. *Experiments with Treble Meniscus Apparatus.*

In the following experiment a different apparatus was used. A is a rectangular cell 20 centims. high, 12·5 centims. long, and 5·5 centims. wide, with front and back of polished plate glass, held together by clamp screws. B and C are glass cups, fitted with lids and tubes as the previously described apparatus (see p. 58), B having one tube and C two (each tube being 15 millims. high and 12 millims. bore). D, E, and F are three similar glass tubes, E and F being provided with platinum electrodes, G and H. The cups were first filled with the coloured liquid and placed in the cell, and the latter filled as in the former apparatus. The object of the experiment was to compare the effects of an up current and of a down current with ordinary diffusion without current in the small tube within D.

Exp. 35.—Lower liquid a saturated solution of cupric nitrate; upper one, 1 volume of nitric acid and 5 of water. A curved line was quickly produced in each of the two tubes, highest up in the one under the anode; that line remained in the colourless part of the liquid whilst the one under the cathode descended or merged quickly into the blue solution. The blue solution rose in height under the cathode, as if the liquid moved in the same direction as the current. Three permanent lines were produced in the tube under the cathode, the top one slightly concave, the two lower ones straight. The line in colourless portion in the tube under anode rose slowly as the lower liquid increased in bulk. No manifest difference of diffusion appeared during 1¼ hours in the tube under the anode when compared with that in the tube with no current, excepting that the liquid had risen more in the former than in the latter, probably in consequence of the heat of conduction resistance.

FIG. 3.



Exp. 36.—The usual double meniscus apparatus (see p. 58) was now used again, and a single series of twenty-five Grove's elements was employed in this and the following experiments to No. 40. The meniscus tubes were 17 millims. high and 8 millims. bore. Lower and upper liquids same as in last experiment. Copious conduction. Almost instantly a line with strong concavity downwards was produced, about 2 millims. above the blue surface of positive meniscus, and another in the diffused portion of blue liquid below the anode. This was followed by the production of two less-defined lines in the blue portion of solution below cathode, and this was immediately succeeded by a strong wave-like motion at positive meniscus, and a projection upwards of portions of the blue liquid, and soon afterward large bubbles of gas or vapour were produced in the middle part of the positive meniscus in the position of the lowest line. On reversing the current the gas was produced strongly in the meniscus under cathode, and continued a short time also at the other meniscus. The violence of the action caused the liquids to mix, and required the experiment to be stopped. The current was too strong for the persistent production of definite lines.

Exp. 37.—Same tubes. Lower liquid, 1 volume of sulphuric acid and 2 of water; upper one, 1 volume of nitric acid and 5 of water. Very copious conduction. A line with convexity downwards was immediately produced at each meniscus, and was soon followed by a strong action at each surface, strongest at the positive one, causing a streaming upwards of the lower liquid as if unequally heated, and a down rushing of the upper liquid, which destroyed the definite line. Small bubbles of gas or vapour were also produced all over the surface of the positive meniscus only.

Exp. 38.—Meniscus tubes 22 millims. high and 12 millims. wide. Lower liquid a mixture of 2 measures of saturated solution of cupric sulphate and 1 measure of dilute sulphuric acid, consisting of 1 volume of acid to $2\frac{1}{2}$ volumes of water; upper liquid, 1 volume of sulphuric acid and 7 of water. Two lines were produced at the negative meniscus. The violence of the action soon put a stop to the experiment. *Remarks.*—The stronger action at the positive meniscus than at the negative one in Experiments 37 and 38 was probably due to the more definite accumulation of acid and basic products in contiguous layers at the former than at the latter meniscus.

Exp. 39.—Meniscus tubes, 17 millims. high and 8 millims. bore. Lower liquid, 1 volume of nitric acid and 1 volume of water; upper one, 1 volume of nitric acid and 5 of water. Conduction copious. A line with convexity downwards produced at once above positive meniscus, followed soon by one above negative meniscus; this was succeeded by strong wavy movements at those surfaces, as if the effect of heat or of liquids of different specific gravity.

Remarks on the Foregoing Experiments.

The phenomena in all the experiments are evidently very complex, and are a mixture of physical and chemical effects. The effects to be explained consist chiefly of the production of the fine lines, and an apparent movement of the mass of the liquid. The hypothesis which seems to be the most completely consistent with them is, that they are chiefly due to electrolytic changes, to differences of specific gravity, to ordinary liquid diffusion, to electric transfer and diffusion of liquids, and to heat of electric conduction resistance. That also the surfaces of mutual contact of the two solutions, and of those solutions with the layers of liquid produced by electrolysis, act in some degree as electrodes and as diffusion diaphragms. That by the electrolytic liberation and subsequent accumulation of electropositive and electronegative ingredients, and the transference of water by electric diffusion and its accumulation on the sides of the surfaces of mutual contact of the liquids, various phenomena, such as layers of liquid of different specific gravities and adhesive powers, lines, layers of colour, alterations of forms of meniscus, downward liquid currents, &c., are produced. That also by the heat of conduction resistance evolved in the tubes in consequence of the reduced area of section of the solutions; by the surfaces of the layers of liquid acting as electrodes; and by the chemical union of acids and bases set free in contact with each other at the meniscuses, thermic expansion and streaming upwards of liquids, &c., are produced. In order to be able to understand the results of each separate experiment, these and the subsequent explanations require to be applied to the particular case.

The apparent transfer of mass may be partly explained upon the assumption that the colouring matter acts as a base towards acids, and as an acid towards bases, and is transferred accordingly, thus producing in the former case, by diminution of colour at the negative and increase of it at the positive end, an appearance of bodily transfer of the heavier liquid in the direction of the current. Others may be explained upon the supposition that basic matter is set free at the liquid cathodes, and acid matter at the liquid anodes; thus, with the lower liquid containing a salt (such as cupric sulphate) composed of a coloured base and a colourless acid, the blue basic matter accumulated at the liquid cathode, and the colourless acid at the liquid anode, and the liquid appeared to move in the direction of the current; but with one containing a salt (such as potassic chromate) composed of a coloured acid and a colourless base, a reverse movement appeared to occur.

The downward movement of the meniscus under the cathode which occurs in some cases (see Experiments 41, 42, and 43), may probably be explained as follows:—By the upward passage of the electric current and the meniscus acting as a kind of diaphragm, a greater volume of liquid was transferred by electric diffusion from the negative to the positive side of the division than in the reverse direction (see "Experiments on Electric Osmose," "Proc. Roy. Soc.," No. 208, 1880, vol. 31, p. 253). This explanation, however, does not appear very consistent with the fact that the lines when first observed are not usually close to the coloured surface of the true meniscus, and do not separate farther from it by continuance of the current.

The influence of chemical composition of the liquids upon the formation of lines is illustrated on page 85. The greater the differences of specific gravity of the two liquids, the more easily were the lines produced. The line occurs more frequently where the current goes from concentrated to dilute solutions of different substances than the reverse; out of 42 lines, 28 were at the positive, and 14 at the negative meniscus. The definiteness of the lines where the current passes upwards, and the indefiniteness usually produced where it passes downwards through the meniscus, may be explained as follows:—In the former case the layer of liquid accumulated above the meniscus by the united action of electrolysis, ordinary and electric diffusion, is of less specific gravity than the layer produced below it, and consequently the two layers do not as readily mix with each other; but in the latter case the layer produced above the meniscus is usually of greater specific gravity than that immediately below the meniscus, and the liquids more readily mix. These circumstances probably also partly explain the greater frequency of the lines at the positive meniscus than at the negative one.

Numerous observations have shown as very probable that the upper line nearly always originates at some distance from the meniscus, and in some cases subsequently moves down to it, whilst in others it separates from it (see Experiment No. 40); and in cases where the two liquids have somewhat mixed, and there is no definite meniscus, the line commences in an extremely dilute and nearly colourless stratum of the lower liquid. The line sometimes commences nearly 5 millims. above the visibly coloured layer.

The apparent thickness of the lines is probably due to convexity. The lines produced are usually straight with a moderate current, and curved with convexity downwards with a strong one. The commonest case is, that the line at first is curved and barely visible, but gradually becomes definite and flat, and remains so. The form of the meniscus is sometimes changed by the passage of the current; this would be a necessary consequence of the alterations produced in the composition and adhesive properties of the layers of liquid on each side of the meniscus. Each newly formed line also possibly acts as a meniscus, a liquid electrode, and a diffusion diaphragm. A series of lines could probably only occur where each successive layer of liquid in ascending order was lighter than the one next below it, and heavier than the one next above it. Such a series occurs more frequently at the meniscus beneath the cathode than in the one beneath the anode.

Whether the meniscus is rendered more permanent or not by temporary passage of an upward current through it, and the accumulation of ions against each of its surfaces, I have not definitely ascertained; the free acid of the upper layer, and the free base of the lower one must, however, subsequently unite and form a third separating stratum close to it.

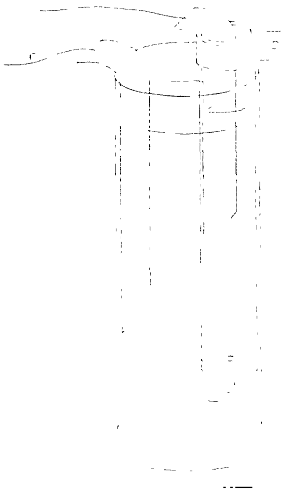
Time is an important element in the formation of the lines; long continued action is, within certain limits, more favourable than a strong current to their production. The first line usually appears in about one to five minutes, the second takes more time, especially if the current has been passing a long time in an opposite direction; the length of time required for the line to appear in the opposite tube on reversal of current, depends upon the period during which the current has been passing in the previous direction, the greater that period the longer must it pass in the other direction. The greater difficulty of producing lines with two liquids, which differ only in degree of concentration (see Experiments Nos. 24, 26, 27, 30, 31, 32, 33, 34, and 39), shows that the rapidity of production of the definite line is not proportional alone to the amount of current passing. I did not ascertain for how long a period the line could be maintained.

The bulk, or form of the mass, of either the lower or upper solution, have little or no effect upon the phenomena, except so far as they affect the electric conduction resistance. It was frequently observed that the lower liquid increased in bulk during an experiment, probably in consequence partly of heat of conduction resistance and partly because of ordinary diffusion transferring more liquid from weak to strong solution than from a strong to weak one, partly also because electric osmose is nearly always more copious from a dilute to a concentrated liquid than the reverse (see "Experiments on Electric Osmose" *loc. cit*). I have not ascertained whether the usually greater electric osmose is wholly or only partly due to ordinary diffusion.

4. *Experiments with single Meniscus Apparatus.*

In order to be able to more perfectly analyse the phenomena by reducing them to their simplest state, I employed in the following experiments an apparatus having only a single meniscus tube (see fig. 4).

FIG. 4.



The outer vessel contained the heavier liquid and the inner cylinder the lighter one, the latter being artificially coloured when necessary by means of litmus. The inner vessel was supported, and capable of being steadily raised by means of a vertical rack and pinion (not shown in the sketch).

In using this apparatus (after placing the electrodes in position), the small cylinder was first raised, and then the exact bulk (viz., 9½ ozs.) of solution was poured into the outer vessel. The inner cylinder being next depressed until its lower end was level with the liquid, the lighter solution was allowed to flow very slowly down the inner surface of that vessel, and the cylinder simultaneously depressed with great regularity (by means of the rack and pinion), so as to keep the meniscus within the small tube, until the vessel was sufficiently immersed. In cases where it was desirable to colour the heavier liquid instead of the lighter one, the meniscus tube projected into the lighter liquid instead of into the heavier one.

This apparatus is more easily managed than the one described on p. 58; the arrangement also offers less conduction resistance, and a less powerful battery suffices; the phenomena obtained with it and requiring to be observed are less numerous, but at the same time less complete; reversal of the current, however, usually enables the remainder of the changes to be perceived. In this apparatus also the phenomena at one meniscus are not liable to be neutralised, or in any other way interfered with by those at the other, this is particularly advantageous with regard to the visible movements. By raising the inner cylinder slightly and then depressing it, a definite meniscus can also be obtained at any time. The following experiments were made with it by the aid of a current from twelve Grove's cells in single series.

Exp. 40.—Meniscus tube 17 millims. long, 7 millims. bore. Outer liquid a saturated solution of sodic sulphate. Inner and coloured one a one-fourth saturated solution of potassic chloride. Current sent up the meniscus tube. Conduction moderate. A line produced at once. The meniscus moved in the direction of the current. Acid accumulated at the upper surface of the meniscus and produced a red layer of liquid about 1·75 millim. thick. By reversing the direction of the current the meniscus descended at once about 2·5 millims. A line with its convexity upwards soon formed in the colourless portion about 4·0 millims. below the border of colour; and another in the coloured portion. There was thus three lines, the middle one being the original meniscus, now somewhat indefinite and dividing the coloured and colourless liquids. By raising the cylinder until a little of the coloured liquid escaped, and then depressing it, a clear definite meniscus was obtained, which soon became indefinite by the influence of the downward current in the tube, and two other lines, one above and one below it, soon began to develop and separate further asunder, as if two distinct layers of liquid (the upper one probably basic and the lower one acid) were accumulated by electric action.

Exp. 41.—Outer liquid, 1 volume of sulphuric acid and 7 volumes of water, sp. gr. of mixture 1·16; inner one 5 volumes of a saturated solution oxalic acid, and 3 volumes of water, sp. gr. of mixture 1·02. Conduction feeble. Current sent up the meniscus tube. The meniscus descended quickly, *i.e.*, in a direction opposite to that of the current. No line (in addition to that of the meniscus) was formed. On stopping the current, the meniscus ascended and, on renewing it, descended again. Reversed direction of current. No line produced. Very little effect, the meniscus descended a little.

Exp. 42.—Inner liquid, 1 volume of a saturated solution of ammonic sulphate, and 3 volumes of water (sp. gr. of mixture 1·08). Outer one, a strong solution of potassic chloride (sp. gr. 1·13). Current was sent up the meniscus tube. Conduction moderate. The meniscus descended rapidly. A line developed in the colourless portion, near meniscus. Under the influence of the current the coloured liquid crept down the sides of the meniscus tube; a colourless portion of liquid of low density also formed at the under surface of meniscus and streamed downwards; on stopping the current these effects ceased, and the meniscus rose. By sending the current now down the tube, a very definite and deeply curved line, with its convexity downwards, was produced. The meniscus soon became flat and descended. Similar streaming of liquid upwards from upper surface of meniscus now took place to that which had occurred downwards; and the layer of liquid immediately upon meniscus, which was previously pink, now became blue, showing that the meniscus acted to some extent like an electrode, in causing the liberation of ions at its surface. Stopped the current; the meniscus became indefinite. Renewed it; the meniscus became definite. Reversed it (*i.e.*, sent it up the tube); the colourless liquid rose in the tube, and the meniscus was very definite.

Exp. 43.—Inner liquid, 1 volume of a saturated solution of sodic hydrate and 3 volumes of water (sp. gr. 1·07). Outer one, a saturated solution of sodic carbonate (sp. gr. 1·11). Current sent up meniscus tube. Conduction very moderate. The meniscus descended at once, and a line was produced, with its convexity upwards, in colourless portion close to the colour. Similar wavy appearances just below the meniscus, and falling of colourless stream as in Experiment No. 42. On stopping the current the meniscus rose and became indefinite and the other line faded away. A definite meniscus was now made by raising and lowering the large tube; and the current was then reversed, *i.e.*, sent down the tube. A line was soon produced with its convexity downwards, in the deep blue liquid; and another in the colourless portion, about 1·75 millim. below meniscus. There was now no wavy appearance or streaming down in the colourless liquid. On stopping the current the convex line ceased and the meniscus was flat. By again reversing the current and sending it down the tube, the liquid descended at once, and two lines were formed, one in the colourless liquid, and the other at the edge of the blue; wavy and falling appearances also occurred again. A layer of liquid of deep blue colour formed upon and above the meniscus. *Remarks.*—I have not found the cause of the reversal of the movement of the meniscus in Experiments Nos. 41, 42, and 43.

Exp. 44.—Inner liquid, 1 volume of saturated solution of sodic carbonate and 3 volumes of water (sp. gr. 1·09). Outer one, a strong solution of ammonic nitrate, slightly acid (sp. gr. 1·19). Current sent up the tube. Conduction very moderate. Definite line produced. The meniscus did not ascend or descend. A layer of red colour was produced in the lowest layer of blue liquid, and around that liquid, showing that the acid set free above the meniscus from the electrolysis of the liquid ascended by capillary action between the blue liquid and the tube. Stopped the current; no such change occurred. Reversed its direction (*i.e.* sent it down the tube). The blue colour became strongly curved with its convexity downwards, the layer of red liquid gradually disappeared, and the meniscus slightly descended in the direction of the current. A wavy appearance of the colourless liquid close to meniscus and streaming of it downwards was produced, as in a previous experiment, as if a heavier liquid was set free at the under surface of the meniscus by electrolysis of the outer solution.

Remarks.—The results of these experiments with the single tube apparatus, may (most of them) be explained in a similar manner (see p. 67, *et seq.*) to the previous ones. They also show that the movements of the meniscus, up or down, depend upon the particular combination of liquids employed, as well as upon the direction of the current: for instance, in Experiment No. 44, no such movement took place; whilst in Experiments Nos. 40, 41, 42, and 43, it occurred freely. The comparatively quick movements of the meniscus in these latter experiments and the circumstances that the contiguous liquid surfaces appear to act as electrodes, favour the view that the movement is partly a result of pressure, and that a somewhat similar movement by the influence of an electric current, takes place to a small extent with miscible liquids in glass tubes as occurs with immiscible ones (such as mercury and an aqueous solution) in a capillary electroscope.

5. *Transfer of the Mass of Solution.*

All the foregoing experiments show that the apparent movement of mass and production of definite lines, is not confined to capillary tubes. With the double meniscus apparatus, the lower liquid appeared to move in the direction of the current in Experiments Nos. 2, 12, 18, 19, 21 (25), 29, 32, and 35; and in the opposite direction in

Experiments Nos. 5, 8, 16, 17 (26?), 30, and 31. It may be remarked respecting this, that in the whole of the latter class, one of the solutions consisted either of sodic carbonate, potassic cyanide, or yellow chromate of potassium, whilst in the former class the only experiment in which an alkaline liquid occurred was No. 18. The apparent movement of the mass in some of this latter class has been already partly explained (see p. 68, *et seq.*).

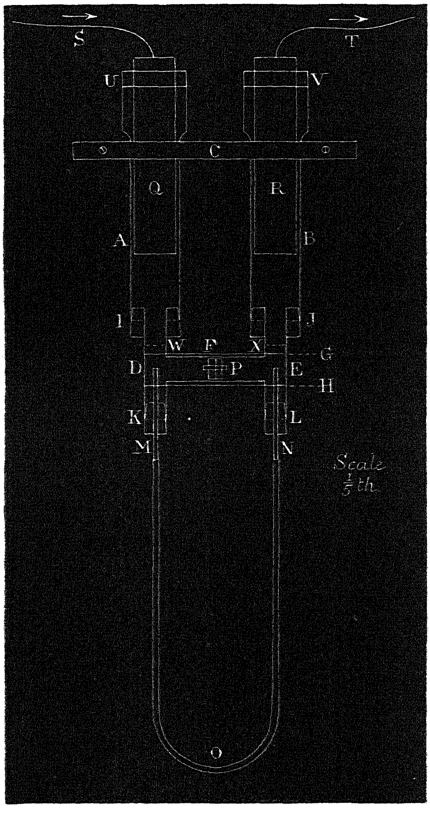
With the single meniscus apparatus, the meniscus moved in the direction of the current in Experiment 40; and in the opposite direction (with certain variations) in Experiments Nos. 41, 42, and 43; whilst in Experiment No. 44, the meniscus moved very slightly. The direction of movement was occasionally reversed during an experiment (see Experiments Nos. 41, 42, and 43). In Experiments Nos. 40, 41, 42, and 43, the movements were quick, as if they resulted from pressure. The slower movements have been already partly explained (see p. 68, *et seq.*).

In order to ascertain more definitely whether there existed a transfer of the body of the heavier liquid due to the passage of the current in the constricted portion of the solution, and also to compare the effects obtained with those recorded of the movement of water by an electric current, as discovered by Armstrong (see "Phil. Mag.," vol. xxxiii, 1843, pp. 194—202), and investigated by Quincke and Jurgensen (see Wiedemann's "Galvanismus," vol. i, 1872, p. 585, or Miller's "Chemical Physics," 4th Edition, p. 530), I employed the following apparatus, fig. 5.

A and B are two glass gas chimneys supported by two transverse and perforated pieces of wood C, joined by screws. D and E are two T-pieces of glass tube, similar, open at all their ends, and joined together by means of a very short india-rubber bung F, and kept from separating by means of two binding wires G, H. These tubes are fixed tightly in A and B by means of perforated india-rubber bungs I and J, their lower ends being closed by perforated corks K and L, through which pass two very narrow glass tubes M and N, open at their ends, and connected by a long vulcanized india-rubber tubing O, of very small internal diameter (about 2 or 3 millims.). The intended use of the india-rubber tube was to allow of a more ready flow of the liquid in the tube P. The bung F is perforated, and a small glass tube P, open at both ends, fits tightly in it. Q and R are two large electrodes of sheet platinum, with terminal wires S and T connected with a battery consisting of a single series of twelve Grove's elements. The vessels are filled throughout with liquid up to the level of the line U and V (W and X are dotted lines, indicating something which appertains to subsequent experiments).

In using the apparatus, a liquid was employed containing a very small amount of minute suspended particles of precipitated sulphur, which had been rubbed up with it, taking care not to use sufficient to block up the finer tubes. After filling the apparatus with the liquid, the tube O was wound tightly upon a narrow cylinder to expel bubbles of air, and then unwound. The apparatus being now placed

FIG. 5.



so that the tube P was in a strong light, the liquid in that tube was closely examined by means of an eyeglass. As soon as the flow of liquid (arising from inequality of level) ceased, the wires S and T were connected with a voltaic battery, and the liquid in the tube again closely watched, in order to detect any flow, both whilst the tube O remained open throughout, and also whilst closed, by means of a pinch-tap.

With the tube P 15 millims. long, and 1·3 millim. internal diameter, and the apparatus filled with a mixture of 19 measures of water, and 1 of pure sulphuric acid, not the slightest horizontal movement of the liquid in the tube could be detected, either during the passage of the current, or immediately afterwards, the only visible movement was that of vertical currents of the liquid outside the tube, evidently caused by change of specific gravity, due to heat of electric conduction resistance produced within the tube.

In three further trials, three solutions consisting of 5, 55, and 255 grs. of potassic cyanide, dissolved in 12 ozs. of water, were employed, but with less effect than in the previous experiment.

To further determine whether there existed a movement of mass due to actions taking place at the mutual contact surfaces of the liquids, I made some experiments with *two* liquids in the apparatus, the mode of operating being as follows:—A heavier coloured liquid (containing suspended sulphur) was poured by means of a long-necked funnel into the tubes D, E, and P, until after expulsion of all air from O, the meniscus of the liquid stood at the level indicated by the dotted lines W and X. Corks (previously fixed on the ends of platinum wires) were now placed rather tightly in the upper ends of D and E, the two electrodes inserted, and the lighter colourless liquid poured simultaneously into A and B, until the level of liquid was exactly the same in each. After all flow of liquid in P had ceased, the corks were withdrawn with extreme slowness, so as not to cause the two liquids to mix at their surface of mutual contact. The further details of manipulation were the same as those previously described.

In a first experiment with two liquids, the lower one consisted of a saturated solution of cupric sulphate, containing suspended particles of sulphur, and the upper one of a mixture of 19 volumes of water and 1 of pure sulphuric acid, and the glass tube P was of the dimensions already given. No movement of the solution in or near P, except those already mentioned, could be detected.

In a second experiment, the lower liquid consisted of 1 measure of strong pure nitric acid, and 7 measures of a saturated solution of cupric nitrate; and the upper one of a mixture of 1 volume of pure nitric acid, and 6 of water. The glass tube P was 15 millims. long, and 2·3 millims. internal diameter. The results were similar to the previous one.

The small tube P was now removed, and a very thin disk of talc, having a minute hole about 1·0 millim. diameter in its centre, was employed in its stead. The disk was fitted water-tight by means of varnish. A current from twenty-five Grove's cells in single series was now used; and a single liquid consisting of 1 measure of sulphuric acid, and $2\frac{1}{2}$ measures of water was employed. Free conduction occurred, and large bubbles of steam were produced on each side of the hole. The current was reversed in direction several times; the tube O being occasionally closed. No translation of the mass due to the current could be detected, the only movements produced appeared to be due to the heat of conduction resistance and the explosive action of the bubbles of steam.

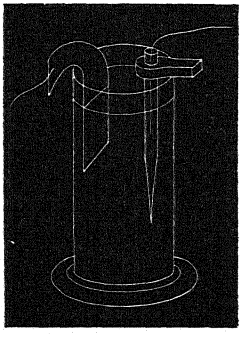
In another experiment I employed two liquids and a current from twelve Grove's cells in single series, the lower liquid being a mixture of 3 volumes of saturated solution of cupric sulphate and 2 volumes of a mixture of 1 volume of sulphuric acid and 2½ volumes of water; and the upper one of 1 volume of sulphuric acid and 7 volumes of water. The action at the two meniscuses produced no visible flow of liquid through the hole in the talc.

From these results, from considerations already stated (see p. 68, *et seq.*), and also from the circumstance that in Armstrong's experiment pure water (a liquid of great electric conduction resistance) was necessary, and in the experiments of other investigators the flow of liquids required badly conducting liquids, and either porous diaphragms or tubes of capillary diameter, I conclude that the bodily flow of the unchanged mass of the heavier liquid in my experiments is, to a large extent, only apparent. The considerable length of time also required for the apparent flow is consistent with this conclusion.

6. *Experiments with Capillary Tubes.*

In order to test the question whether a similar to-and-fro movement by an electric current takes place with miscible liquids in capillary tubes, as occurs with mercury and an aqueous solution in a capillary electroscope, and also to ascertain whether the quicker movements of the liquids in Experiments Nos. 40, 41, 42, and 43 was of this kind, I made a number of experiments with a small apparatus similar to fig. 4 (see p. 70), having (in place of the inner cylinder and tube) a *capillary* tube, fig. 6 (containing the lighter liquid) immersed in the heavier solution, the lighter liquid being very strongly coloured with "mauve" aniline dye. The electric currents employed were from a

FIG. 6



single cell of copper and platinum in dilute sulphuric acid, and from a single series of twelve Grove's elements. The electrodes were of platinum, and the capillary tube was capable of being raised or lowered through very minute distances by means of the rack and pinion. Tubes of various internal diameters were used, varying from 1·, ·6, ·45, to ·35 millim. The only pair of solutions employed consisted of a one-fourth saturated solution of potassic chloride as the lighter liquid, and a saturated one of sodic sulphate as the heavier one (as in Experiment No. 40). The currents were only continued during periods of less than half a minute at the utmost.

With a tube 1 millim. diameter, and the electric current from either source, or in either direction, no movement of the meniscus was visible. With a tube ·6 millim. diameter, or with one ·45 millim. diameter, either an upward current or a downward one from the twelve cells caused a downward motion of the meniscus, strongest with the upward current. The motion was more rapid than in the experiments with the usual wider meniscus tubes of this research, and less rapid than with mercury in a capillary electroscope; it appeared to be due partly to heat of conduction resistance (producing expansion) and partly to electric convection. (N B.—Electric osmose would have produced a more rapid movement of the meniscus upward by a downward current, than of downward movement by an upward one.) Solid particles suspended in the liquid were accelerated in their velocity of falling by the passage of the current. The meniscus did not continue very definite, and no lines (if any existed) were sufficiently distinct to be perceived. The meniscus also showed no return movement on stopping the current. (In this respect it differed from the movement with mercury, &c.) I could not ascertain that the meniscus influenced the motion; the latter continued the same when the meniscus was at the extreme end of the tube as when it was within it. It is probable that the movements were to some extent of the same kind as those observed by Armstrong, Quincke, and Jurgensen (see p 74), and would have occurred with one liquid alone. The current from the single cell had either no effect, or else a barely perceptible one similar to that produced by the one from the Grove's battery. With a tube ·35 millim. bore, a very slight downward movement was produced by the upward current from the battery. The downward current had the same effect, but in a less degree. No effect was observed with the current from the single cell.

From the comparative slowness of the movement and the great strength of electric current necessary to produce it, I consider that nearly the whole of the movement of two miscible liquids in capillary tubes is very different from that occurring with mercury and solution in such tubes. The tubes, however, used with mercury are usually of much smaller diameter.

The results obtained with the single meniscus apparatus, and with the capillary tubes, support the conclusion that there is a small amount of real transfer of the bulk of the liquid otherwise than by diffusion. The rapidity of such movement appeared to vary, being slowest in wide tubes with feeble electric current, quicker in the single meniscus tube and in the capillary tube experiments, but in no case was it as rapid as the movement of the mercury in a capillary electroscope. The conditions of motion observed in these cases differ, however, considerably from those in the experiments of Armstrong, Quincke, Jürgensen, and Wiedemann. In their experiments badly conducting liquids only were used, and no movement occurred if good conducting solutions were employed; in mine it only took place in the latter liquids.

7. *Relation to Electric Osmose.*

In order to be enabled to judge more correctly respecting the influence of "electric osmose," I made a number of experiments similar to that performed by Porrett ("Annals of Phil.," vol. viii, p. 74). The results of these experiments have been made the subject of a separate paper (see "Experiments on Electric Osmose," *loc. cit.*), for more convenient reference by future investigators.

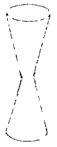
As in all those experiments (numbering 68) except one, the liquid was observed to flow in the same direction as the positive electricity, and showed no signs of reversal of movement with solutions of potassic cyanide, yellow potassic chromate, acid chromate of potassium, or sodic carbonate, I again conclude that the apparent movement of the lower liquids in my experiments with a double meniscus apparatus is of a considerably different character from that known as "electric osmose."

I also made experiments of electric osmose with the same pairs of liquids as those used in the single meniscus apparatus in Experiments Nos. 40 to 44. The electric currents were sent from strong to weak solution in the osmose experiments corresponding to Nos. 40, 41, 42, and 44, and from weak to strong in that agreeing with No. 43. In each case the greatest osmotic transfer occurred in the direction of the current, and was strong in No. 42 and weak in No. 44. These directions of movement do not agree with those obtained in the experiments referred to, and the results indicate that the direction of the diffusion at a liquid meniscus is largely affected by the presence of a diaphragm.

8. *Influence of Form and Dimensions of the Meniscus Tube.*

Experiments Nos. 40 to 44 were repeated with a meniscus tube of the form of an hour-glass (fig. 7), and having an internal narrowest diameter of about 3 millims., but no new results were obtained.

FIG. 7.



One influence of the dimensions of the meniscus tubes consisted in enabling a suitable magnitude of liquid surface to be presented to the action of the current; a sufficiently narrow tube in relation to the power of the current was necessary in order to enable the effects to be produced quickly. Another effect of narrow tubes was to produce conduction resistance, and thereby give rise to evolution of heat. The dimensions of the meniscus tube have also several effects, some of which appear to be independent of the meniscus itself; for instance, the narrower the tube the greater is the ratio of surface of tube to the mass of the liquid, and consequently the relatively greater are the effects due to surface action, such as the quick mechanical movements of the mass due to electric convection.

9. *Influence of Molecular Structure of the Liquids.*

That the relatively different molecular structures of the opposed liquids lies at the basis of the phenomena, and are the static conditions which permit the uniform cause (viz., the electric current) to produce different effects at the surfaces of contact, to those it produces in the homogeneous mass of each liquid is evident, because the special local effects (such as lines and layers) were usually more conspicuous in proportion to the degree of physical and chemical differences of the two solutions; for instance, they were more difficult to produce and more feeble when two portions of the same acid, but of different degrees of dilution, were employed (as in Experiments Nos. 24, 27, 32, 33, and 39), than when one of the liquids was a dilute acid and the other a solution of a heavy metallic salt.

10. *Influence of Cohesion, Viscosity, Specific Gravity, Adhesion, Ordinary Diffusion, &c., of the Liquids.*

The different degrees of cohesion, fluidity, viscosity, and specific gravity of the liquids, probably all affect the phenomena; the greater the difference of specific gravity between the two liquids the more easily were the lines produced, and the more distinct usually were the chief phenomena. Some liquids, however, such as syrupy solution of phosphoric acid, by being too viscous, as well as badly conducting, prevented the effect.

Both the absolute and relative degrees of adhesion of the two liquids to the tubes, especially in very narrow ones, must also affect the results. In a separate paper on the "Phenomena of the Capillary Electroscope," "Proc. Roy. Soc.," No. 209, 1881, I have shown that adhesion performs an important part; and that mercury flows more rapidly from a capillary glass tube into dilute sulphuric acid when an electric current is passing in the same direction than when it is not passing; and it would be desirable, therefore, to examine the influence of adhesion upon the rates of flow of electrolytes (as well as of mercury) through tubes of glass (and other material) whilst under the influence of an electric current.

That the phenomena of lines and movement of mass of liquid in these experiments are not wholly capillary is proved by the circumstance that they have been produced in tubes of as large a diameter as 15 millims. Capillary action, however, affects the results (see p. 78).

Assuming that the explanation I have offered of the phenomena (see p. 67, *et seq.*) is correct, ordinary diffusion is affected during the passage of the current, the whole of the molecules being subjected to electric influence, which probably aids diffusion at the one meniscus and neutralizes it at the other. A result obtained in the experiments on electric osmose, viz., that the liquid flow is more rapid when the direction of electric osmose coincides with that of ordinary osmose, than when it is opposite to it, supports the conclusion that ordinary diffusion (although suspended) influences the results. In Experiment No. 15 the dilute nitric acid diffused into the cupric bromide; this diffusion was by the influence of the current decreased at the negative meniscus and increased at the positive one; an opposite effect to that exerted by the current upon water. The interference of ordinary diffusion might probably be largely obviated by using liquids of equal specific gravity.

The less rapid osmose which occurs where an electric current passes from a strong (through a diaphragm) to a weak solution, than where it passes in the reverse direction, is not likely to be due to difference of chemical action, because at each junction of the liquids in the cells divided by porous partitions (see "Proc. Roy. Soc.," vol. 31, p. 253), there was opposed equivalent quantities of acid and bases set free by electrolysis, and therefore equal chemical action. Much more investigation, however, remains yet to be made in this part of the subject. Although the experimental evidence of an influence of the current upon ordinary diffusion without a porous diaphragm is not extensive, it is sufficient in Experiments Nos. 40, 41, 42, and 43 to prove that diffusion of liquids is greatly affected by the passage of a current, and that it varies with the nature of the liquids and with other circumstances not yet determined. The electric current, by liberating electrolytic products at the two surfaces of each meniscus, must also further affect ordinary diffusion and electric osmose at the meniscus.

11. *Influence of Miscibility of the Liquids.*

It would be interesting to examine the influence of non-miscible electrolytes upon each other under the usual conditions in the single meniscus apparatus, if a pair of suitable ones could be found. I attempted to pass a current from a single series of twenty-five Grove's cells through two immiscible liquids lying in contact with each other in a U glass tube 10 millims. diameter. One liquid consisted of water saturated with ether and mercuric chloride, and the other of ether saturated with water and that salt. No perceptible current passed, or other visible effect was produced.

12. *Influence of Light, Heat, and Temperature.*

I have not observed that the phenomena are in any way related to the influence of luminous rays; nevertheless, as light is well known to affect chemical union, it is not unreasonable to suppose that it influences (though in a very much less degree) mechanical mixture.

The influence of temperature of the liquids upon the phenomena has not been experimentally examined, partly because no valuable addition of knowledge was likely to accrue, and partly because rise of temperature is a disturbing condition in the case, and it would be difficult to manipulate with heated liquids. The effects of heat of conduction resistance interfere in nearly every instance with the detection and accurate observation of the other phenomena, especially in cases where a powerful current circulates. By the influence of such heat, the lower liquid expands, and thus appears to increase in volume by diffusion. When the temperature is higher, wavy lines are produced at the meniscuses, especially at the positive one, and as the heat increases, streams of upflow of the lower solution or of downflow of the upper one occur; and finally, bubbles of steam or gas are produced at the positive meniscus, and stop the experiment. The heat evolved appears to be mostly at the contiguous liquid surfaces, probably because acids and bases are transferred there by the current and chemically unite; it is greatest at the positive meniscus, because the relative densities of the acid and basic layers there tend to localise the effect.

13. *Influence of Electric Conditions.*

With the exception of ordinary liquid diffusion, adhesion, and capillary action, all the effects are primarily due to the electric current, and therefore dependent upon conduction and magnitude of flow. The chief locality of action is at and near the meniscuses, apparently also to a less extent at the surface of contact of the liquids with the meniscus tube; but exactly in what manner the current operates at the contact surfaces is a difficult question.

The effects of the current are both physical and chemical. A uniform cause or homogeneous force (such as an electric current is commonly supposed to be), whilst acting under uniform conditions is usually considered to produce uniform immediate effects. In the present case, however, it appears as if one portion of the electric current produces direct chemical decomposition of the solution; a second immediately disassociates water from saline matter by diffusion, and a third produces direct electric convection, or simple mechanical movement of the liquid; or the diffusion may be viewed as a result of electric convection. Either then the total current of uniform property acts not under uniform conditions, but in several different ways, producing effects which require different degrees of electromotive force to produce them, or the current consists of several portions of electricity of different electromotive force. The supposition that these different effects require currents of different electromotive force to produce them, is in harmony with the generalised idea, that instances of mechanical union, or mere solution of solids in liquids, merges into those of definite chemical union by insensible degrees, and the two classes thus form an unbroken series, ranging from those of liquids which mix in every degree with each other, to those of the most powerful chemical combinations, in which the ingredients unite only in rigidly definite proportions.

With regard to the purely physical effects, viz., electric osmose and transfer of mass, we may reasonably refer them to electric convection attending electric discharge between surfaces in opposite electrical states, the charged condition of those surfaces being dependent upon the special local resistance to electric transfer which always exists in greater or less degree at the surfaces of contact of bodies of different physical and chemical structure. In capillary tubes the charged surface of the glass becomes also an important circumstance. The unequal transfer of acids and bases must also affect the results. Acids always travel in greater proportion towards the anode than the metals to which they have been united move towards the cathode, provided the solutions are not kept quite neutral.

And with regard to the electro-chemical effects, ions are probably liberated at every surface of junction of liquids of sufficiently different composition through which the current passes. A sufficient difference of property of the two bodies (in addition to liquidity and a sufficient electromotive power of the current) appears to be a necessary condition of electrolysis; with lesser differences probably only electric osmose is produced. (I have not examined the effects of electric currents passing in oblique directions through a surface of contact of two solutions of equal conductivity but of different specific gravities.) The layers of liquid on each side of each meniscus are manifestly results of electrolysis, and the lines are probably due to them; but why a line should originate in the midst of a homogeneous liquid, at a distance from a definite meniscus and approach (or be approached by) the meniscus, and merge into it, I have not ascertained. Sometimes, however, the lines separate from the meniscus (see Experiment No. 40).

Although the chain of conducting particles from one metallic electrode to the other was as perfect with the two liquids as with one, there existed a difference of conditions at the surfaces of liquid contact to those existing in the mass, and one effect of this variation is a sudden difference of resistance to electric transfer at that locality. As also the amount of electric diffusion is greater when the current passes in one direction than in the other in experiments of electric osmose, we may reasonably predict as probable that the conduction resistance to currents of opposite direction passing through such an arrangement would be similarly unequal. It is probable also that the amounts of this *per saltum* resistance and difference of resistance, would vary with every different pair of liquids. These are subjects of further investigation.

As a perfect meniscus acts to some extent as an electrode, an imperfect one must do the same, though in a less degree. The positive one (or that below the cathode) acts more perfectly in this respect than one below an anode, because the action of electrolysis tends to maintain it, and even make it more definite. It would further appear that every inequality of composition, or of internal structure of the liquid in the path of the current must also act to some extent as an electrode; and if this be correct, not only the meniscus proper, but also the surfaces of the layers of acid and basic products of electrolysis next the masses of the two solutions must also act in like manner; but this is only a supposition for future examination.

As I have not been able to obtain any visible lines or movements of the liquids with an electric current of insufficient power to produce electrolysis, and electrolysis will reasonably account for several of the phenomena, I conclude that electrolysis is not merely a concomitant circumstance, but acts also as a cause.

The converse phenomena, viz., the influence of electric currents on the diffusion of liquids, has been made the subject of separate experiments ("Electric Currents produced by Liquid Diffusion and Osmose," "Proc. Roy. Soc.," vol. 31, p. 296).

I have not searched for any new action of magnetism upon the phenomena of lines, movements of liquids, &c.

14. *Influence of Chemical Action.*

As purely chemical action appears to be only a coincident and not a fundamental part of the phenomena, the inferences to be drawn from it are probably less important. Chemical action is a constant concomitant in the case; it always took place, 1st, by electrolysis, 2nd, by contact of liberated ions; and, 3rd, occasionally by contact of the original liquids. It sometimes occurs at the meniscus previous to the passage of the current. As also, by the influence of the current, acids and bases are liberated at the contact surfaces, and as in the foregoing numerous experiments with the double and single meniscus apparatuses, the kind of solutions in mutual contact have been found to largely influence the phenomena of the lines, &c., so the layers of new compounds formed by electro-chemical action probably also have similar effects.

From the results of the experiments with the meniscus apparatuses it also appears that the lines may be produced with almost every possible chemical combination of electrolytes; for instance, where the current goes from concentrated to dilute solution, whether from acid salt to acid (as in Experiments Nos. 1, 2, 3, 6, 7, 8, 9, 10, 11, 12, 14, 15, 19, 21, 23, 25, 28, 29, 35, 36, and 38); one acid to another (Experiments Nos. 37 and 41); one alkaline salt to another (Experiment No. 17); an acid salt to a neutral one (Experiments Nos. 4, 13, and 22); a neutral salt to an alkaline one (Experiments Nos. 5 and 44); an alkaline salt to a neutral one (Experiments Nos. 16 and 20); one neutral salt to another (Experiments Nos. 40 and 42); or from an alkaline salt to an alkali (Experiment No. 43); but not so readily from a strong solution of an acid to the same diluted (Experiments Nos. 24, 27, 32, 33, 34, and 39); nor from a strong one of an alkaline salt to the same diluted (Experiments Nos. 26, 30, and 31). A possible relation of the apparent movement of the mass of the liquid to the chemical composition of the two liquids has been already indicated (see p. 68).

"Phenomena of the Capillary Electroscope." By G. GORE, LL.D., F.R.S. Received November 23, 1880. Read January 6, 1881.

In a communication "On the Capillary Electroscope" ("Proc. Roy. Soc.," vol. 30, p. 32), I have described various details necessary to be attended to in the construction and use of a modified form of that apparatus, and I now give an account of an investigation I have made of the phenomena of the movements of the mercury in such instruments. A research I formerly made, "On the Movements of Liquid Metals and Electrolytes in the Voltaic Circuit" ("Proc. Roy. Soc.," vol. 10, p. 235), throws additional light upon the subject. Some of the phenomena arising out of this research have, for the sake of convenience, been made the subjects of separate communications. (See "Effects of Electric Currents on the Surface of Mutual Contact of Aqueous Solutions," "Proc. Roy. Soc.," vol. 30, p. 322, *ibid.*, vol. 31, p. 250; "Influence of Electric Currents on Diffusion of Liquids;" "Experiments on Electric Osmose;" "Electric Currents caused by Liquid Diffusion and Osmose," *ibid.*, vol. 31, p. 253, *ibid.*, vol. 31, p. 296.)

Erman, in the year 1809, appears to have been the first to observe the movements of mercury in a conducting solution while under the influence of an electric current. Since that time a large number of investigators have examined the phenomenon and the allied ones of electric osmose, electro-capillary action, electric currents produced by capillarity, the mechanical effect of electric currents upon liquids and upon solid particles suspended in them, &c. Among these are Armstrong, E. Becquerel, Buff, H. Davy, Draper, Du Bois Reymond, Faraday, Heidenhain, Hellwig, Herschel, Hittorf, Jürgensen, Kühne, Lippmann, Logeman, Matteucci, Paalzow, Pfaff, Poggendorff, Porrett, Quincke, Reichert, Reuss, Runge, Sabine, Sérullas, Varley, Wheatstone, Wiedemann, and Wright. It is difficult, therefore, to entirely avoid restatement of some of the results arrived at by these investigators.* I have examined the movements in relation to a variety of conditions, some of which, however, are unessential and may be eliminated or diminished. The phenomena have been found to be purely physical, except in those cases where the electricity was of too high tension and produced electrolysis, and in those in which the solution acted chemically upon the mercury.

1. *Influence of the Kind and Strength of the Solution.*

The following experiments were made with the above-mentioned form of apparatus, except in the instances otherwise described. The electric current employed was usually (unless otherwise stated) derived from two wires, one of copper and the other of platinum, each about 1 millim. diameter, immersed about 4 or 5 millims. deep in spring water.

Experiment No. 1.—1 oz. of water, 40 grs. of potassic fluoride. Motion occurred freely, and in the same direction as the current.

Exp. 2.—Various solutions of potassic chloride from 20 to 60 grs. per oz. of water were tried, and in each case the movements of the mercury were in the same directions as those of the current; the former of these solutions was too weak, it offered too much conduction-resistance, and the latter too strong, it clogged the tube. With a solution composed of 1 oz. of water and 40 grs. of potassic chloride in a tube of fine bore strong movements occurred, and 85 millims. of extra height of mercury pressure was required to bring the meniscus back to its original position and counterbalance the effect of the current.

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* A brief translation of the researches of Jürgensen, Quincke, and Wiedemann, may be found in W. A. Miller's "Chemical Physics," 4th edition, p. 530.

Exp. 3.—1 oz. of water and 40 grs. of potassic iodide. The direction of the motion of the mercury agreed with that of the current. 51 millims. height of mercury pressure was required to balance the influence of the current.

Exp. 4.—1 oz. of water and 40 grs. of potassic bromide. All the results the same as those described with the chloride. The current required the pressure of 82 millims. height of mercury to balance it.

Exp. 5.—1 oz. of water and 40 grs. of potassic carbonate. The direction of the movement of the mercury was the same as that of the current. The current required 83 millims. height of mercury to balance it.

Solutions of cyanide of potassium were tried of various strengths from 10 to 100 grs. per oz. of water; the weak ones yielded feeble movements in the direction of the current and the stronger ones clogged the capillary tube.

Exp. 6.—1 oz. of water and 33 grs. of cyanide of potassium in a very fine capillary. An upward current sent the mercury down a long distance and out at the end of the tube. The reverse directions of movement obtained with solutions of potassic cyanide compared with those obtained with dilute sulphuric acid, in capillary tubes, agree with those obtained with the same liquids, and a large globule of mercury in an open shallow dish (see "Proc. Roy. Soc.," vol. 10, p. 235).

Exp. 7.—With a solution of 40 grs. of the cyanide in 1 oz. of water, the movement was more free than with 50 grs. of that salt. By previously passing a downward current, the commencement of motion by the subsequent up-current appeared to be retarded. In another apparatus, consisting of a column of mercury about 20 centims. total height, with a long capillary surmounted by a pressure-chamber quite filled with mercury, and closed by a glass plug, an upward current caused the meniscus to move downwards slowly more than 25 millims., and a downward one raised it about 8 or 10 millims.

Exp. 8.—1 oz. of water and 50 grs. of potassic cyanide. In the fine part of the tube the motion of the mercury was opposite to that of the current, but in the *coarse* part of the tube I noted that it moved in the direction of the current.* When the mercury dropped from the end of the capillary into this solution it formed chains of globules which, when about 2 or 3 millims. high, fell over and formed a heap like a pile of brambles or sticks; the chains of globules did not break.

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* This movement requires investigation.

Exp. 9.—1 oz. of hydrocyanic acid ("Scheele's strength") and 40 grs. of potassic cyanide: motion feeble. The directions of motion of the mercury in a capillary of wide bore were the same as those of the current; but in one of narrow bore an upward current sent the mercury freely down. A downward current had but little effect, but on closing the circuit, or on insulating it immediately after an upward current had been passed, the mercury rose instantly.

Exp. 10.—1 oz. of water, 40 grs. of potassic cyanide, and 10 grs. of bicyanide of mercury, formed a very good liquid. The mercury moved in the same directions as the current.

Exp. 11.—1 oz. of water and 40 grs. of mercuric cyanide. A current from copper and platinum wires in dilute sulphuric acid produced little or no movement, but the charge from an ebonite electrophorus (the opposite platinum wire of the electroscope being connected "to earth") caused the mercury to move either up or down in the same direction as the discharge.

Note.—The electrophorus yielded electricity of much too high tension for use with this instrument, especially if the mercury in the capillary tube was the negative pole; gas then quickly collected at the meniscus and stopped the action.

Exp. 12.—1 oz. of water and 40 grs. of mercuric chloride. This solution was unsuitable.

Exp. 13.—A mixture of 7 volumes of distilled water and 3 of strongest aqueous ammonia. The movements were very feeble, but in the usual direction, both with the water-cell and the electrophorus. The mercury would only emerge from the end of the capillary in large drops, not in the usual stream of minute ones, probably in consequence of the small adhesion of aqueous ammonia to mercury (see "Adhesion of Liquids to Mercury," "Phil. Mag.," August, 1863).

Exp. 14.—1 oz. of water and 40 grs. of potassic sulphate. An upward current sent the mercury up freely, and a downward one sent it down. Charging the upper electrode by the electrophorus depressed the mercury as usual; but charging the lower electrode also depressed it, and more strongly. This exceptional effect was obtained several times.

Exp. 15.—1 oz. of water and 40 grs. of potassic borate. The movements of the mercury were feeble, but in the usual direction, both with the current and electric discharge.

Exp. 16.—1 oz. of water and 40 grs. of acid carbonate of sodium. Free movements of the mercury took place in the usual directions, both by the current and the discharge.

Exp. 17.—1 oz. of water and 40 grs. of sulphate of sodium. Feeble movements were produced in the usual directions, both by the current and the discharge.

Exp. 18.—Dilute sulphuric acids of different strengths, varying from 1 in 40 to 1 in 10. Numerous experiments were made with different forms of apparatus, both horizontal and vertical, employing both the feeble current from the water-cell and the charge from the electrophorus. In every case the movement of the mercury was in the same direction as the current and the electric discharge.

Exp. 19.—1 oz. of water and 10 grs. of boracic acid. Feeble movements occurred in the usual directions with the voltaic current; but charges from the electrophorus, whether applied to the upper platinum wire or to the lower one, depressed the meniscus freely. (Compare Exp. 14.)

Exp. 20.—With strong aqueous acetic acid feeble effects only were obtained, either with the water-cell or electrophorus, but the movements were in usual directions.

Exp. 21.—1 oz. of water and 75 grs. of racemic acid. Rather feeble movements, but in the usual directions, were obtained, both with the current and the discharge.

Exp. 22.—Alcohol. No visible movement was produced by the current from the water-cell, and only a very feeble downward one, by charging the upper electrode with the electrophorus.

Exp. 23.—1 oz. of water and 5 grs. of sodic hyposulphite. The solution was too dilute and was otherwise unsuitable.

Exp. 24.—1 oz. of water and 5 grs. of ammonic alum. Source of current, the water-cell. The movements were very feeble, but in the usual directions; and the downward movements were much more quick and were larger than the upward ones. With 10 grs. of the salt per ounce the movements were more free, but the solution was still too dilute.

Exp. 25.—1 oz. of water and 5 grs. of potassic sulphite. The effects were exactly the same as with the weakest solution of ammonic alum.

Exp. 26.—1 oz. of water and 40 grs. of sulphate of zinc. Free movements occurred in the usual directions.

In the following experiments a drop of dilute sulphuric acid had been previously added to the exciting solution of the voltaic cell.

Exp. 27.—1 oz. of water and 40 grs. of cupric sulphate. Similar results occurred to those with the zinc salt. No signs of deposited copper or of viscosity of the meniscus were produced by the upward current.

The following solutions were also tried:—1 oz. of water as the solvent, and 40 grs. of the solid were employed in each case; ammonia alum; potassic sulphite; potassic sulphocyanide; potassic ferrocyanide; glacial phosphoric acid; sodic nitrate; potassic nitrate; ammonic nitrate; and sodic chlorate. Movements in the usual directions occurred in every instance, and no special phenomena were observable. A solution of 40 grs. of potassic ferrid-cyanide produced a film upon the mercury.

All these experiments show that the movements are greatly affected both by the kind and strength of the solution.

2. *Influence of the Relative Dimensions of the Mercurial Surfaces.*

In order to ascertain whether the movements were dependent upon the circumstance that the mercurial surfaces were of very unequal dimensions, I immersed the extremities of the capillary ends of two vertical electroscopes (of the form I have usually employed) in a solution of 40 grs. of glacial phosphoric acid in 1 oz. of water, with about 10 millims. in length of the capillaries filled with that liquid. I then passed a current (from a large cell of copper and platinum plates in town water) down one capillary tube and up the other; the meniscus descended in the former and ascended in the latter, and in each instrument the motion was just as free as if one of the electrodes had a large surface.

I also passed a current through a short column of mercury in a vertical capillary glass tube between two portions of conducting solution in contact with platinum wires. With a solution of 40 grs. of potassic chloride in 1 oz. of water, both below and above the mercury in a tube of fine bore, and a current derived from the copper and platinum wires immersed in a mixture of 1 volume of sulphuric acid and 19 of water, the mercury moved by a jerk in the direction of the current. To ascertain whether electro-dynamic induction affected the motion in this experiment, I included in the circuit one wire of a double coil of about 1,300 turns of insulated copper wire ("No. 29"), wound upon an iron axis, and closed the secondary circuit. The motion of the mercury was diminished.

The various experiments in which a short column of mercury was between liquids, such as dilute sulphuric acid, solution of potassic chloride or cyanide, in a horizontal capillary, a stronger current was required to move it than when it was in the form of a long column in the usual vertical instrument; it also moved by jerks and only a small distance. A reason why a column of mercury between portions of aqueous solution in a capillary tube of the usual diameter, required a current of greater electromotive force to move it, was probably because the longer total length of the slender column of solution offered much greater conduction resistance.

With a solution of 1 oz. of water, and 33 grs. of cyanide of potassium at each end of a column of mercury about 25 millims. long in a coarse and horizontal capillary tube, with platinum electrodes, and a much stronger current from the copper and platinum wires immersed in a solution composed of 60 grs. of cyanide of potassium and 1 oz. of water, the mercury moved to and fro, each way about 25 millims., and in reverse directions to those of the current. The mercury would not move by the influence of a current from the same wires excited by a mixture of 1 volume of sulphuric acid and 19 of water. I ascertained by experiment that the former current had the greatest electromotive force.

1 oz. of water and 40 grs. of potassic cyanide, and a current from copper and platinum wires in the solution of cyanide (60 grs. per oz.) above mentioned. With a short length of mercury in a horizontal tube less coarse than the last, the mercury moved in each direction opposite to the current a distance of about 13 millims.

Also by employing a platinum electrode without any mercury upon it, below the vertical capillary, all the movements occurred as usual. These latter experiments prove that the lower electrode need not be composed of a fluid metal, and therefore that I might have omitted to make the previous one respecting the influence of relative dimensions of the two mercurial electrodes.

3. *Influence of Molecular Structure, Cohesion, Liquidity, Viscosity, and Pressure upon the Movements.*

That the molecular structures of the mercury and solution affect the phenomena is quite certain, because we know that the physical properties in general of substances depend essentially upon their internal architecture; the intimate structure of bodies, however, is so inscrutable, that investigation has not yet disclosed it clearly to us. As a solid metal in place of the mercury will not admit of either of the movements, the molecular motion in each liquid is a *relative* one; the motion of the one liquid depends upon the liquid state of the other. The movement is also dependent upon the kind of molecular structure of the solution: 1st, because it varies in direction with a variation in the kind of solution: and 2nd, because it varies also with a variation of strength of the solution; and in each case without any evidence of chemical or electro-chemical action.

Cohesion of the mercury influences the movements, and tends to cause that substance to move more readily from a narrower to a wider part of the tube than in the reverse direction.

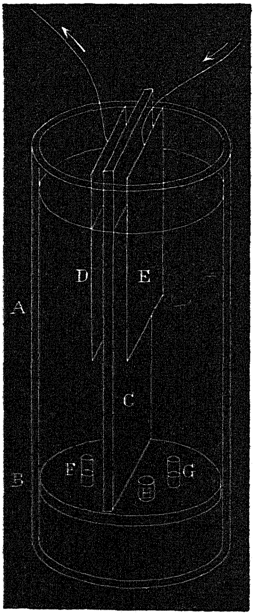
As the liquid state of the lower electrode was not a necessary condition of the motions, the movement of one electrode is not at all dependent upon a similar movement of the other. A single liquid electrode is sufficient to exhibit all the phenomona. Fused salts might probably be used with success, instead of aqueous solutions. (See "Movements of Liquid Metals and Electrolytes in the Voltaic Circuit," "Proc. Roy. Soc.," vol. 10, par. 10.)

The movement was constantly found to diminish as the viscosity of the solution was increased beyond a certain point, by addition of saline or other solid or viscous substance. Pressure probably has but little influence, because liquids are so slightly compressible.

4. *Relation to Immiscibility of the Liquids.*

Is it a necessary condition of the movements that the two liquids must be incapable of mixing with each other? In the year 1859 (see "Proc. Roy. Soc.," vol. 10, p. 235, par. 9) I made some experiments bearing upon this question: "1st, a definite layer of oil of vitriol was placed beneath a layer of distilled water weakly acidulated with sulphuric acid, and the terminal wires (from a voltaic battery) immersed in the upper liquid; no visible movements occurred at the boundary line of the two liquids; 2nd, a dense solution of cyanide of potassium was placed in a small glass beaker, a few particles of charcoal were sifted upon its surface, and a layer of aqueous ammonia half an inch deep carefully poured upon it. A vertical diaphragm of thin sheet gutta-percha was then fixed so as completely to divide the upper liquid into two equal parts; the vessel was placed in a strong light, and two horizontal platinum wire electrodes from sixty-six pairs of freshly charged Smie's cells immersed one-eighth of an inch deep in the liquid ammonia on each side of the diaphragm. A copious current of electricity circulated, but no movements of the liquids at their mutual boundary line could be detected. A small globule of mercury placed in the lower liquid at once produced evident signs of motion."

Fig. 1.



In order to test in a more searching manner whether during the passage of an electric current movements occur at the boundary surfaces of two liquids which are miscible with each other, similar to those which take place at the mutual surfaces of a liquid metal and electrolyte; and also to ascertain whether any translation of the mass of an aqueous solution occurred similar to the bodily movement of the mercury, I employed the following apparatus (see fig. 1).

A is a thick glass cup about 4 centims. wide and 10 centims. high, having an accurately and tightly-fitting horizontal division of gutta-percha B; also a similarly well-fitting vertical one C. D and E are electrodes of sheet platinum. F and G are thin glass tubes about 10 millims. high, and 3 millims. diameter, open at both ends, and fixed liquid-tight in holes in B. H is a plug of beeswax.

In preparing to use this apparatus, the division B was first fixed in water-tight, the space below it filled with a nearly saturated aqueous solution of cupric nitrate, and the plug H inserted; the height of liquid being adjusted so as to be about half way up the glass tubes. Corks fixed upon the ends of platinum wire were now placed in the tubes. The glass vessel was then nearly filled with a mixture (at 60° F.) of one measure of sulphuric acid and 10 of water; the electrodes immersed; the division C inserted and made as water-tight as possible; and the corks removed very carefully.

An electric current from a single series of five or six Grove's elements of one pint capacity each being now passed through the liquids, the cupric solution appeared to *descend* slowly in one tube, and ascend in the other, moving in the same direction as the positive electricity, and occupying a period varying from five to twenty minutes for the movement. As the phenomena observed in this kind of experiment were considerably different from those observed with mercury in capillary tubes, I have made a separate examination of them under the title of "The Influence of Voltaic Currents on the Diffusion of Liquids" ("Proc. Roy. Soc.," vol. 31, p. 250). As also I could not readily obtain definite lines of separation of two miscible liquids in tubes of as small a diameter as those employed with mercury, I have been unable to solve as satisfactorily as I could have wished the question whether perfect immiscibility of the liquids is a necessary condition of the movements of the mercury in such experiments; Herschel stated ("Phil. Trans.," 1824, p. 189) that it is.

5. *Relation of the Movements to the Volume and Specific Gravity of the Mercury.*

In order to ascertain whether the whole of the mercurial column moved in the direction of the motion of the meniscus, or expanded and contracted coincident with the advance and retreat of that surface, I made experiments with a series of different capillary electroscopes in a *horizontal* position, taking care to remove an additional part of the apparatus in each successive experiment:—

The first electroscope (No. 1) consisted of two similar capillary tubes, inserted in two pressure-chambers, connected together in a direct line by a long straight glass tube of narrow bore, each chamber being provided with a platinum wire terminal. The chambers, connecting tube, and capillaries were filled with mercury; air was excluded, and the ends of the capillaries were immersed as usual in dilute sulphuric acid containing electrodes of mercury. In the second apparatus (No. 2) the intermediate glass tube was removed, and a single pressure-chamber, 60 centims. in length (with a platinum wire at each end), was used in its stead. In a third (No. 3), the chamber was reduced to a length of about 8 centims., and had a platinum wire at each end. In a fourth (No. 4), the chamber was removed altogether, and the central tube and capillaries were formed of a single piece of glass, the middle part being about 4 centims. long, of the usual diameter and bore, and having a fine wire of platinum sealed into it as an electrode.

I also made experiments with a series of electroscopes in a *vertical* position, each successive apparatus being increasingly simple, and each closed at the upper end and perfectly filled with mercury, except the lower end containing the electrolyte.

The first (No. 1*a*) consisted of the capillary (dipping as usual into dilute sulphuric acid with a mercury electrode), short pressure-chamber, and a long glass tube above it, closed at the top by a thick india-rubber tube and a strong pressure-clip. In the second (No. 2*a*), the long glass tube was dispensed with, and the upper end of the pressure-chamber was closed by a glass plug. In the third (No. 3*a*), the chamber was excluded, the platinum wire electrode being melted into the side of the large part of the glass tube, and the upper end of that tube drawn out to a fine point, and closed by melting sealing-wax whilst mercury was exuding from it. The fourth (No. 4*a*) was similar to the third, except that the upper end was closed by fusing a part of the fine tube itself whilst it was filled with mercury. And in a fifth (No. 5*a*), the bulky portion of the glass tube was dispensed with by fixing an extremely fine platinum wire in the somewhat larger and upper part of the capillary by fusion.

In all the experiments made with these different instruments, whether horizontal or vertical, the electrolyte used was a mixture of 1 volume of pure sulphuric acid and 14 of water, and the voltaic current employed was that already mentioned (see p. 86). In each of the experiments with them the mercury moved in the same direction as the current, whether that was towards the point of the capillary tube or the reverse. In many of the experiments a charge from an ebonite electrophorus was used as the source of electricity, and in these cases also the mercury moved in the direction of electric discharge. With the horizontal electroscopes (all of which were open at both ends), the mercury only moved at that end at which the current or discharge passed; but if the current or discharge passed at both ends simultaneously, then the mercury moved at both ends simultaneously, whether the electric flow was from end to end, from both ends to middle, or from middle to both ends (provided it was free to move), and the directions of the motion in the two ends agreed with the above general statements.

In these various experiments the amount of capillary movement with a given current did not appear to vary greatly with the total bulk of mercury in the entire apparatus.

The displacement of volume of the mercury being so extremely small, it may be accounted for on the supposition that minute traces of air (not visible by means of an eye-glass) adhere to the sides even of the smallest and most perfectly prepared tubes; and when by the action of the current or discharge, the meniscus is caused to approach the capillary orifice, these minute portions of air become rarefied. This view is supported by the facts, that traces of air are continually observed in the capillaries, and also that when the apparatus was reduced to its simplest form and smallest size, as in No. 5*a*, the motion of the meniscus appeared to be somewhat less free.

From these results, I consider it improbable that the movements are to any large extent attended by a change of volume of the mercury.

6. *Influence of Adhesion, Capillarity, and Surface Tension.*

In a paper "On the Adhesion of Liquids to Mercury" ("Phil. Mag.," vol. 26, 1863), I have stated that "if a drop of Nordhausen sulphuric acid, about one-tenth of an inch in diameter, is carefully placed by means of a glass rod upon the centre of a clean globule of pure mercury, about 80 grs. in weight, it instantly diffuses itself in a thin film over the surface of the metal, and the mercury becomes flattened, and exhibits vertical movements all over its surface; but if the experiment is made with a strong aqueous solution of ammonia or of caustic potash, no such results occur, the alkaline solution contracts itself into a spherical form, and persistently floats to the side of the mercury without spreading itself over the surface."

This circumstance, viz., the small adhesion of aqueous ammonia to mercury, is probably connected with another already mentioned (see p. 88), viz., that when the mercury was caused to flow out of the end of the capillary tube into a solution of ammonia, it would only emerge in large drops instead of the usual stream of minute ones. Difference of degree of adhesion of different solutions to mercury, however, will not by itself account for the movements.

Further, it is well known that if a drop of water is placed upon a horizontal surface of mercury, and an electric current passed from the mercury into the water, the two liquids spread out, whilst if the current is reversed the water contracts, as if in the former case the adhesion between the two substances was increased, and in the latter decreased; and various interpretations have been given of these phenomena (see Sabine, "Phil. Mag." vol. 2, 1876, Supp., "On Electricity disengaged between Mercurial Surfaces"; section 2). I have repeated this experiment: 1st, with dilute sulphuric acid, the effects were the same as with water; 2nd, with a solution of potassic cyanide and a downward current the solution spread out, and with an upward current no such effect was manifested; and 3rd, with one of potassic carbonate the solution spread out, both with an upward current and with a downward one, most with the former. These changes of adhesion of liquids to each other by electric influence cannot alone produce the to-and-fro movement in the electroscope, because no action between two bodies could cause both of them to simultaneously advance or retreat together in the same direction.

These apparent changes of adhesion of liquids to each other by the influence of electric currents are, however, essentially similar to the phenomena under consideration, and agree with the chief and wider truths of electrolysis, viz., that when a metal is made positive to a liquid by the passage of an electric current from the former into the latter, there is usually excited a tendency to union of the two bodies; and that, when made negative by a current, an opposite tendency is excited. They also agree with the fact that if, whilst mercury is slowly issuing from the end of the electroscope into a solution, the mercury is rendered positive to the liquid by the passage of an electric current, the rapidity of flow of the metal is usually increased, while, if it is rendered negative, the flow is usually decreased. The degrees of rapidity of flow of mercury from the end of a capillary glass tube into different electrolytes, with and without the simultaneous passage of an electric current, might form a subject of investigation.

That the phenomena of the electroscope are largely affected by the adhesion of the liquids to the tubes, especially when the movements of the liquids take place in capillary tubes of extremely small diameter, does not admit of doubt, because the magnitude of the surfaces of adhesion then bear a larger proportion to the mass of the liquid in the capillary. It is well known that some of the most fundamental properties of liquids are changed whilst under the influence of adhesion in capillary spaces. For instance, Melsens found that, notwithstanding the great volatility of bisulphide of carbon when alone, it required more than an hour to expel 2 or 3 cub. centims. of that liquid from a mixture of 30 grms. of charcoal and 23 cub. centims. of the sulphide contained in a glass tube immersed in boiling water (see "Phil. Mag.," July, 1877, p. 43).

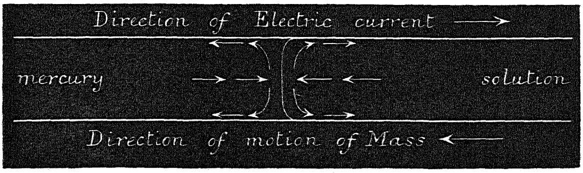
It is evident also that the movements must be greatly affected by the relative degrees with which each of the liquids adhere to the containing tube, and it is not improbable that as the aqueous solutions adhere to the glass tube and the mercury does not, the latter yields to the moving influence to a very much greater extent than the former, and that the motion is chiefly (if not wholly) produced through the agency of the adhesion of the solution to the tube, and not so much through that of the mercury.

Although the motion is frequently prevented by adhesion of the mercury to the tube, it is not caused simply by diminution of that adhesion whilst under the influence of the current, because if that did occur, it could only operate by allowing some dynamic cause to produce the motion.

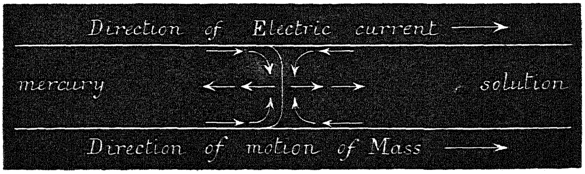
The evidence with regard to adhesion supports the hypothesis that the motion is primarily due to a direct mechanical action at the immediate surfaces of contact of the mercury and solution, producing movements of solution and mercury in directions agreeing with those in the annexed figures;* and the to-and-fro movement of the mass is a secondary circumstance, arising from the former in consequence of the greater adhesion of the solution than of the mercury to the tube.

FIG. 2.

In KCy solution.


Direction of Electric current ⟶
mercury
solution
Direction of motion of Mass ⟵

In dilute sulphuric acid.


Direction of Electric current ⟶
mercury
solution
Direction of motion of Mass ⟶

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* The repulsive action being usually at the negative pole in liquids, is a similar fact to the molecular repulsion of highly rarefied gases at the negative electrode in Mr. Crookes's experiments.

The mechanical law of action and reaction of equal and opposite forces also requires the primary movements of the mercury and solution to be in opposite directions. If this hypothesis is correct, the unequal adhesion of the mercury and solution to the glass is a necessary condition of the simultaneous advance of the two liquids in the same direction. If the tube was as free to move as the liquids, it would of course move in an opposite direction to them. The number of physical actions, however, involved in the case is probably much greater than is represented by the above explanation.

Herschel observed that "the peculiar action is only exerted at the common surface of the fluids;" and nearly all the evidence supports the conclusion that it is primarily an action of films and not of the mass of mercury or of solution within the capillary tube.

Even when during the passage of a current, the column of liquid is prevented by adhesion from rising or falling in the capillary tube, there is probably a motion occurring in the contiguous portions of the mercury and solution, though not usually visible even by the aid of a microscope.

Armstrong's experiments ("Phil. Mag." vol. 23, 1843, pp. 194–202) also indicate that the movement of the mercury is affected by an action of adhesion between the surface of the liquid and glass within the tube, because in his experiments, when the silk thread, acting as a fulcrum, was removed, the amount of liquid flowing in one direction was equal to that flowing in the other, and there was no manifest transfer of the mass.

In all cases with a globule of mercury in a pool of liquid, the outer film of mercury and the film of liquid in contact with it appears to move in the same direction; the mass of the liquid, and also that of the interior of the mercury, must, therefore, of necessity, move in a direction opposite to that of the films (compare Herschel, "Phil. Trans." 1824, p. 165).

The opposite movements obtained with dilute sulphuric acid and with solution of potassic cyanide might also be explained on another supposition, viz., that one of these liquids adheres more strongly, and the other less strongly, than mercury to glass; but this does not appear to be a fact.

Although the phenomenon of to-and-fro motion appears to be essentially due to a combination of surface actions, it is not confined entirely to capillary spaces, the motion (as is well known) takes place as readily with a large globule of mercury in a pool of liquid as in a capillary tube. (See also "Movements of Liquid Metals and Electrolytes in the Voltaic Circuit," "Proc. Roy. Soc.," vol. 10, par. 35.) The to-and-fro movement, in the case of a globule of mercury in a pool of liquid, is dependent upon the circumstance that the solid surface of the containing vessel, upon which the mercury and film of solution beneath it lie, constitutes a fulcrum upon which the moving layers of liquid mechanically act. If it were possible to suspend the mercury in the midst of a large mass of a liquid electrolyte of its own specific gravity, the effects would probably be greatly modified, and there would be very little translation of the mass of mercury (compare Herschel's paper, section 10, p. 167).

In the experiments by Armstrong, also those of Quincke and Jürgensen, of the mechanical transfer of liquids by the passage of electric currents through slender columns of them in tubes, the axial portion of the liquid was observed to move in an opposite direction to the outer layer. This inner moving portion, or that farthest away from the surfaces of adhesion, may be regarded as the return current.

It is generally considered that the surface layer of particles of every solid and liquid is in a state of mechanical tension, and consequently that every mass of such substance, even that which is only of microscopic magnitude, may be crudely viewed as being bounded by a more or less tightly-fitting envelope. This circumstance also appears to be related to the phenomenon of motion of the mercury. The surface tension of that metal appears to be lessened at the positive mercurial electrode, and increased at the negative one in nearly all electrolytes. As there is also a surface tension of the liquid as well as that of the mercury, the primary motion is probably a resultant of the two. According to this view there exists *two* modes by which an electric current may vary the surface tension of a conductor in contact with an electrolyte, the one being attended by electrolysis, and the other not. If it were simply a result of diminution of surface tension of the mercury by the passage of an outgoing current, then it ought not to vary in direction by variation of liquid, strength of liquid, or diameter of tube.

As adhesion influences so greatly the action of the instrument, perfect freedom from dust is a most important condition of success, and the most effectual way of securing this is to insert the capillary tube in the pressure-chamber as soon as it is made, and fill it at once with clean mercury. Multitudes of points of adhesion are met with in tubes which have long been open to the atmosphere. The pressure tube should also be completely freed from dust by previously agitating successive portions of mercury in it. Dust, not moisture, is the great source of failure. Adhesions of the mercury do not usually interfere unless they happen to be within the range of movement of the meniscus.

The influence of friction I have not examined; it plays, however, an important part in the practical use of the instrument.

7. *Relations of the Movements to Heat and Temperature.*

As heat, applied to the junction of mercury and an electrolyte, produces an electric current, it is reasonable to infer that an electric current passed through such a junction in the same direction as the one produced, would tend to lower the temperature. And as it is well known that both the cohesion of liquids and their elevation are diminished by rise of temperature, it is probable that the electric current, by producing a minute change of temperature at the junction, affects to a slight extent the capillary elevation. By comparing, however, the directions of the electric currents obtained by heating mercury in contact with various solutions which exercise no chemical action upon it (see "Thermo-Electric Behaviour of Aqueous Solutions with Mercurial Electrodes," "Proc. Roy. Soc.," vol. 29, p. 472), with the directions of movements produced by given directions of electric currents in the same solutions in these experiments, it may be perceived that they do not to any large extent coincide.

That the mercury should also conversely suffer slight changes of temperature by electro-capillary action, is in accordance with the discovery made by Pouillet ("Ann. de Chemie et de Phys.," vol. xx, 1822, pp. 141–162), that heat is evolved by the capillary absorption of liquids by solids; and with the experiments of Jungk ("Phil. Mag.," vol. 2, 1876, p. 454; and "Pogg. Ann.," vol. cxxv, p. 292), and further, with those of Melsens, who states that by mixing 4·45 grms. of charcoal and 33 of bromine, the rise of temperature was 35° ("Phil. Mag.," vol. 2, 1876, p. 454; also "Mém. de l'Acad. Royal de Belgique," vol. xxiii).

The movement is evidently not caused by heat of conduction resistance, because that would produce expansion and advancing motion only. If also the motion is simply due to thermic expansion, then heat of chemical combination at the anode in cases of electrolysis in the capillary ought to cause the mercury to move downwards, but with solutions of potassic cyanide it moves the reverse. The relations of electro-capillary movements to heat require, however, much more investigation.

8. *Relations of the Movements to Electric Conditions.*

The phenomena of the capillary electroscope are not results of electrolysis, nor of disruptive discharge, but of conduction proper, and may occur either with or without electro-chemical change. In nearly all previous investigations of the electric movements of mercury, very much stronger electric currents were employed, and the results were complicated by electrolytic phenomena. That electrolysis, when it does occur, is only a coincident circumstance, is proved by the non-liberation of hydrogen at the meniscus when that surface is made negative in dilute sulphuric acid and in various other solutions; also, by the mercury at the meniscus not becoming viscid when made the negative pole in a solution of cupric sulphate (see

Exp. No. 27); and further, by the surfaces of the meniscus not becoming oxidised when made the positive pole in either of the various solutions usually employed; provided in all these cases that ordinary chemical action is absent and that the current is sufficiently weak.

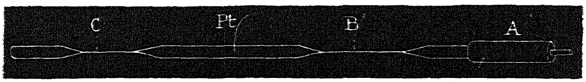
The reverse directions of movement produced by the same direction of current in dilute sulphuric acid and in solution of potassic cyanide, probably cannot both be explained by the theory that the movements are due to electro-chemical exidation and deoxidation. The upward movement caused by a downward current can hardly be due to electrolysis, because that action, by continually destroying the outer film of mercury, tends to diminish its surface tension.

Being largely dependent upon electric conduction, the amount of movement of the mercury is affected by every circumstance which alters the conductivity. The part of the circuit which offers the greatest amount of conduction resistance is the slender column of solution between the meniscus and the end of the capillary tube; there the resistance is considerable. The nearer, therefore, the meniscus is to the end of the tube, and the shorter the column of solution (as well as that of the mercury), the more rapid is the movement, especially if the tube is slightly larger in diameter towards that end; this circumstance also tends to make the downward movement an accelerated one, even in a tube of uniform diameter, and the upward movement the reverse; and also accounts for the fact that the instrument is more sensitive to a downward current than to an upward one, unless the tube becomes narrower downwards at too rapid a rate.

The secondary current of an induction coil was not suitable for working the instrument, because it produced electrolysis. On many occasions an electrophorus was used as the source of electricity, and this also produced a similar effect. On charging the electroscope with it, either by induction or by contact, the movements were freely produced, provided the electroscope was not insulated. If it was perfectly insulated the movements did not occur, and if it was imperfectly insulated and then charged by momentary contact, the mercury continued to run out at the end of the capillary tube after removal of the electrophorus, and ceased to flow by discharging the instrument.

I made a capillary tube in accordance with the annexed sketch, with a pressure tube A, and a platinum wire electrode Pt; and filled

FIG. 3.



it with mercury whilst in a horizontal position; then melted it off at the point B by means of a minute flame; broke it off at C, and fixed it vertically with its end C in dilute sulphuric acid above an electrode. Not a trace of air was visible in any part by the aid of a strong magnifying glass. By charging the upper electrode by induction with an electrophorus, the mercury descended freely; and by charging the lower one it ascended, provided the free end was not insulated. The motion of the mercury, therefore, is evidently not produced by mere electric charge but requires electric flow.

Even a residual electric charge of the ebonite base of the reverser (see "Proc. Roy. Soc." vol. 30, p. 32) at the surfaces of contact of that substance with the brass fittings which had been connected with the poles of the nearly exhausted little water-cell was sufficient to work the instrument. Also a voltaic current which raised the meniscus 19·5 millims. would not produce a visible movement of the needle of a torsion galvanometer having a coil of 100 ohms resistance. These facts illustrate the extreme sensitiveness of the instrument to electric flow of the feeblest tension; and the apparatus might be used for examining the conductivity proper of electrolytes.

From the results achieved by Quincke ("Pogg. Ann.," vol. cxiii, 1861), and by Jürgensen (Reichert and Du Bois Reymond's "Archiv," 1860, p. 573; also "Chemical Physics," by W. A. Miller, 4th Edition, pp. 530–533), and the various results obtained by myself, I conclude that the primary mechanical movement in the instrument is due to a more or less charged electric state of the surfaces of the liquids and tube. The electric tension accompanying that state alters the degrees of adhesion of the substances to each other, and produces electric convection, which, in consequence of the unequal adhesion of the solution and mercury to the tube, produces a to-and-fro movement of the mass. The less fundamental results, such as reversal of direction of movement with solutions of potassic cyanide, or with tubes of different diameter, would probably be found to be necessary results of the above causes under the altered conditions. A brief translation of Quincke's explanation of somewhat similar phenomena may be found in W. A. Miller's "Chemical Physics," 4th Edition, p. 532.

The primary action appears to consist nearly wholly of a direct conversion of electricity into mechanical power; and this accords with the great sensitiveness of the instrument and the comparatively considerable force of the movements. This force has been already applied by Lippmann in the construction of an electro-capillary engine. The converse action, viz., the production of electric currents by mechanically raising and lowering mercury and an acid solution in capillary tubes, has also been obtained by Lippmann ("Phil. Mag.," vol. 47, 1874, p. 281).

9. *Influence of Chemical Action.*

The movements do not appear to depend upon the chemical nature of the solution, because they take place equally well with acid, alkaline, and neutral liquids. Being also purely physical, they are not dependent upon chemical action; such action, when it does occur, appears in every case to interfere with them.

Note.—Since the publication of a previous communication "On the Capillary Electroscope" ("Proc. Roy. Soc.," vol. 30, p. 32), I have been favoured by M. Lippmann with the following remarks respecting that instrument. "1st. The liquid is to be diluted sulphuric acid, containing something like one-third its weight of sulphuric acid. Weak acid does not film glass properly; most liquids do not; and then stoppages, or a jumping motion of the mercury, occur, such as you have described. 2nd. The capillary tube is to be cut very short (to about 10 millims.), the motions are in that case ten times more rapid than in a tube of 10 centims., because the friction is reduced in that proportion; besides, possibilities for obstruction are reduced also in the same proportion. 3rd. The instrument is only fit for measuring electromotive forces smaller than one Daniell; by using over-great electromotive forces the capillary constant goes over its maximum value, and then the movement of the mercury is reversed as you noticed it to be the case at the end of your communication (see, about this maximum, 'Ann. Chimie et Physique,' 1875, and also 1877). If you will do me the pleasure of visiting M. Jamin's laboratory in the Sorbonne, you will find there several electrometers in good working order; three of these are being used by different observers for separate researches, with a precision of $\frac{1}{10000}$ Daniell."

May 5, 1881.

THE PRESIDENT (followed by THE TREASURER) in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates recommended for election into the Society were read from the Chair, as follows:—

Ayrton, Prof. William Edward.
Bates, Henry Walter.
Bristowe, John Syer, M.D., F.R.C.P.
Christie, William Henry Mahoney, M.A., Sec. R.A.S.
Dickie, Prof. George, A.M., M.D., F.L.S.
Kempe, Alfred Bray, B.A.
Macalister, Prof. Alexander, M.D., Sec. R.I.A.
McLeod, Prof. Herbert, F.I.C., F.C.S.
Phillips, John Arthur.
Preece, William Henry, C.E.
Samuelson, Bernhard, M.I.C.E.
Stoney, Bindon Blood, M.A., M.I.C.E.
Traquair, Ramsay H., M.D.
Watson, Rev. Henry William, M.A.
Wright, Charles R. Alder, D.Sc.

The following Papers were read:—

I. "On the Determination of the Ohm in Absolute Measure." By Lord RAYLEIGH, F.R.S., and ARTHUR SCHUSTER, Ph.D., F.R.S. Received April 12, 1881.

Part I.—By Lord RAYLEIGH.

It is generally felt that considerable uncertainty still attaches to the real value of the ohm, or British Association unit of resistance. The ohm was constructed to represent 10^9 C.G.S. absolute units, but according to Kohlrausch* it is nearly 2 per cent. too great, and according to Rowland† nearly 1 per cent. too small. On the other hand, H. Weber‡ has obtained by more than one method results very nearly in harmony with those of the British Association Committee. Influenced partly by the fact that the original apparatus (though a good deal out of repair) and the standard coils themselves were in the Cavendish Laboratory, I determined last June to repeat the measurement by the method of the Committee, which has been employed by no subsequent experimenter, and sought permission from the Council of the British Association to make the necessary alterations in the apparatus. In this way I hoped not merely to obtain an independent result, but also to form an opinion upon the importance of certain criticisms which have been passed upon the work of the Committee.

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* "Phil. Mag.," xlvii, p. 294, 1874.

† "American Journal of Science and Arts," 1878.

‡ "Phil. Mag.," v, p. 30, 1878.

The method, it will be remembered, consists in causing a coil of insulated wire, forming a closed circuit, to revolve about a vertical axis, and in observing the deflection from the magnetic meridian of a magnet suspended at its centre, the deflection being due to the currents developed in the coil under the influence of the earth's magnetism. The amount of the deflection is independent of the intensity of the earth's magnetic force, and it varies inversely as the resistance of the circuit. The theory of the experiment is explained very fully in the reports of the Committee,* and in Maxwell's "Electricity and Magnetism," section 763. For the sake of distinctness, and as affording an opportunity for one or two minor criticisms, a short statement in the original notation will be convenient:—

H=horizontal component of earth's magnetism.

γ=strength of current in coil at time t.

G=total area inclosed by all the windings of the wire.

ω=angular velocity of rotation.

$\theta=\omega t$=angle between plane of coil and magnetic meridian.

M=magnetic moment of suspended magnet.

ϕ=angle between the axis of the magnet and the magnetic meridian.

K=magnetic force at the centre of the coil due to unit current in the wire.

L=coefficient of self-induction of coil.

R=resistance of coil in absolute measure.

$MH\tau$=force of torsion of fibre per unit of angular rotation.

The equation determining the current is—

$$L\frac{d\gamma}{dt}+R\gamma=HG\omega\cos\omega t+MK\omega\cos(\omega t-\phi) \quad . \quad . \quad . \quad (1),$$

whence

$$\gamma=\frac{\omega}{R^2+L^2\omega^2}\{GH(R\cos\theta+L\omega\sin\theta)$$

$$+KM(R\cos(\theta-\phi)+L\omega\sin(\theta-\phi))\} \quad . \quad (2).$$

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* Collected in one volume. London, 1873.

If L were zero, or if the rotation were extremely slow, the current would (apart from KM) be greatest when the coil is passing through the meridian. In consequence of self-induction, the phase of the current is retarded, and its maximum value is diminished. At the higher speeds used by the Committee, the retardation of phase amounted to 20°.

To find the effect of (2) upon the suspended needle, we have to introduce MK and the resolving factor $\cos(\theta-\phi)$, and then to take the average. This, on the supposition that the needle remains on the whole balanced at ϕ, must be equal to the force of restitution due to the direct action of the earth's magnetism and to torsion, *i.e.*, $\mathrm{MH}\sin\phi+\mathrm{MH}\tau\phi$. Thus—

$$\frac{\frac{1}{2}\mathrm{MK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{GH}(\mathrm{R}\cos\phi+\mathrm{L}\omega\sin\phi)+\mathrm{KMR}\}-\mathrm{MH}(\sin\phi+\tau\phi)=0.$$

In the actual experiment τ is a very small quantity, say $\frac{1}{1000}$; and the distinction between $\tau\phi$ and $\tau\sin\phi$ may be neglected.

$$\mathrm{R}^2-\mathrm{R}\frac{\frac{1}{2}\mathrm{GK}\omega\cot\phi}{1+\tau}\left(1+\frac{\mathrm{MK}}{\mathrm{GH}}\sec\phi\right)+\mathrm{L}^2\omega^2-\frac{\frac{1}{2}\mathrm{GKL}\omega^2}{1+\tau}=0 \quad . \quad . \quad (3).$$

If we omit the small terms depending upon τ and upon MK/GH, we get on solution and expansion of the radical—

$$\mathrm{R}=\tfrac{1}{2}\mathrm{GK}\omega\cot\phi\left\{1-\frac{2\mathrm{L}}{\mathrm{GK}}\left(\frac{2\mathrm{L}}{\mathrm{GK}}-1\right)\tan^2\phi-\left(\frac{2\mathrm{L}}{\mathrm{GK}}\right)^2\left(\frac{2\mathrm{L}}{\mathrm{GK}}-1\right)^2\tan^4\phi\ldots\right\} \quad . \quad . \quad . \quad . \quad (4).$$

The term in $\tan^4\phi$ is not given in the report of the Committee, but, as I learn from Mr. Hockin through Dr. Schuster, it was included in the actual reductions. But the next term in $\tan^6\phi$, and one arising from a combination of the correction for self-induction with that depending on M, are not altogether insensible, so that probably the direct use of the quadratic is more convenient than the expansion. At the high speeds used by the Committee the correction for self-induction amounted to some 8 per cent., and therefore cannot be treated as very small.

If the axis of rotation be not truly vertical, a correction for level is necessary. In the case of coincidence with the line of dip, no currents, due to the earth's magnetism, would be developed. If the upper end of the axis deviate from the vertical by a small angle β towards the north, the electromotive forces are increased in the ratio $\cos(\mathrm{I}+\beta):\cos\mathrm{I}$, *i.e.*, in the ratio $1+\tan\mathrm{I}.\beta$, I being the angle of dip. A deviation in the east and west plane will have an effect of the second order only. The magnetic forces due to the currents will not act upon the needle precisely as if the plane of the coil were always vertical, but the difference is of the second order, so that the whole effect of a small error of level may be represented by writing G $(1+\tan I.\beta)$ for G in (3) or (4).

The next step is to express GK in terms of the measurements of the coil. In order that there may be a passage for the suspending fibre and its enveloping tube, it is necessary that the coil be double, or if we prefer so to express it, that there be a gap in the middle. If

a=mean radius of each coil,

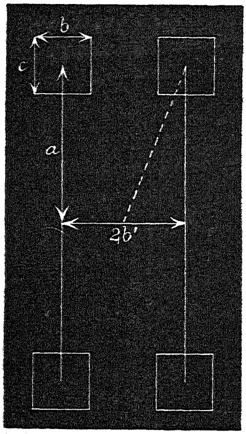
n=whole number of windings,

b=axial dimension of section of each coil,

c=radial dimension of section of each coil,

b'=distance of mean plane of each coil from the axis of motion,

α=angle subtended at centre by radius of each coil, so that $\cot\alpha = b'/a$,



then—

$$G=\pi n a^2\left(1+\tfrac{1}{12}\frac{c^2}{a^2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5),$$

$$K=\frac{2\pi n}{a}\sin^3\alpha\left\{1+\tfrac{1}{24}\frac{c^2}{a^2}(2-15\sin^2\alpha\cos^2\alpha)\right.$$
$$\left.+\tfrac{1}{24}\frac{b^2}{a^2}(15\sin^2\alpha\cos^2\alpha-3\sin^2\alpha)\right\} \quad . \qquad (6),$$

so that

$$GK=2\pi^2 n^2 a\sin^3\alpha\left\{1+\tfrac{1}{6}\frac{c^2}{a^2}+\tfrac{5}{8}\frac{b^2-c^2}{a^2}\sin^2\alpha\cos^2\alpha-\tfrac{1}{8}\frac{b^2}{a^2}\sin^2\alpha\right\} \quad . \qquad (7).$$

The correction due to the finiteness of b and c is in practice extremely small, but the factor $\sin^3\alpha$ must be determined with full accuracy.

In order to arrive at the value of MK/GH, which occurs in (3), we observe that the approximate value of K/G is $2\sin^3\alpha/a^3$; so that

MK/GH is equal to $\tan\mu$, where μ is the angle through which the needle of a magnetometer is deflected when the suspended magnet (M) is placed at a distance from it $a/\sin\alpha$ to the east or west, with the magnetic axis pointing east or west. In practice the difference of readings when M is reversed is taken in order to double the effect, and any convenient distance is used in lieu of $a/\sin\alpha$, allowance being easily made by the law of cubes.

The correction for torsion is determined by giving the suspended magnet one (or more) complete turns, and observing the displacement. If this be δ_1, reckoned in divisions of the scale, *i.e.*, in millimetres, and D be the distance from the mirror to the scale reckoned in millimetres,

$$\tau=\frac{\delta_1}{4\pi D} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

The correction for scale reading, necessary in order to pass from $\frac{1}{2}\tan 2\phi$ to $\tan\phi$, will be explained under the head of reductions.

Corrections depending upon irregularity in the magnetic field, and in the adjustment of the magnet to the centre of the coil are given in the report. They are exceedingly small. The same may be said of errors due to imperfect adjustment of the coil with respect to the axis of rotation.

In remounting the apparatus the first point for consideration was the driving gear. The Committee used a Huyghens' gearing, driven by hand, in conjunction with a governor. This, it appeared to me, might advantageously be replaced by a water-motor; and Bailey's "Thirlmere" engine, which acts by the impulse of a jet of water upon revolving cups, was chosen as suitable for the purpose. As the pressure in the public water pipes is not sufficiently uniform, it was at first intended to introduce a reducing valve; but on reflection it seemed simpler to obtain a constant head of water by connecting the engine with a small cistern at the top of the building. This cistern is just big enough to hold the ball-tap by which it is supplied, and gives at the engine a head of about 50 feet.

The success of this arrangement depends upon attention to principles, as to which it may be well to say a few words. The work done by many prime movers is within practical limits proportional to the speed. If the work necessary to be done in order to overcome resistances, as n overcoming solid friction, or in pulling up weights, be also proportional to the speed, there is nothing to determine the rate of the engine, and in the absence of an effective governor the motion will be extremely unsteady. In general the resistance function will be of the form—

$$Bv+Cv^2+Dv^3+\ .\ .\ .\ ,$$

in which the above-mentioned resistances are included under B. The term in C will represent resistances of the nature of viscosity, and that in D a resistance such as is incurred in setting fluids in motion by a fan or otherwise. By these resistances, if present, the speed of working will be determined.

In the water impulse engine, however, the work is not proportional to the speed. At zero speed no work is done; neither is any work done at a speed such that the cups retreat with the full velocity of the jet. The speed of maximum efficiency is the half of the last, and the curve representing work as a function of speed is a parabola with vertex directed upwards. If we draw upon the same diagram the curve of work and the curve of resistances, the actual speed will correspond to the point of intersection, and will be well or ill defined according as the angle of intersection is great or small. At the higher speeds of the coil (four to six revolutions per second) so much air is set in motion that the resistance curve is highly convex downwards, and no difficulty is experienced in obtaining a nearly uniform motion. But when the speed of rotation is as slow as once a second, the principal resistance is due to solid friction, and the requisite curvature in the diagrams must be obtained in the curve of work. It was necessary in order to obtain a satisfactory performance at low speeds to introduce an additional reducing pulley, so that the engine might run fast, although the coil was running slow.

The revolving coil with its frame, and the apparatus for suspending the magnet, were at first arranged as described by the Committee. This description, with drawings, is to be found in the report, and it is reproduced in "Gordon's Electricity and Magnetism," vol. i. The water engine was ready about the middle of June, and towards the end of the month the apparatus was mounted by Mr. Horace Darwin. During July and August preliminary trials were made by Mr. Darwin, Mrs. Sidgwick, and myself, and various troubles were encountered.

The only point in which the arrangement adopted by the Committee was intentionally departed from was in the connexion of the magnet and mirror. The magnet is necessarily placed at the centre of the revolving coil, but in their arrangement the mirror is on the top of the frame and is connected to the magnet by a brass wire. In order to save weight, I preferred to have the magnet and mirror close together, not anticipating any difficulty from the periodic and very brief interruption caused by the passage of the coil across the line of sight. A box was, therefore, prepared with a glass front, through which the mirror could be observed, and was attached to the end of a brass tube coming through the hollow axle of the coil. This tube itself was supported on screws resting on the top of the frame. The upper end of the suspension fibre was carried by a tall tripod resting independently on the floor.

The first matter for examination was the behaviour of the magnet and mirror when the coil was spinning with circuit open. At low speeds the result was fairly satisfactory, but at six or more revolutions per second a violent disturbance set in. This could not be attributed to the direct action of wind, as the case surrounding the suspended parts was nearly air-tight, except at the top. It was noticed by Mr. Darwin that even at low speeds a disturbance was caused at every stroke of the bell. This observation pointed to mechanical tremor, communicated through the frame, as the cause of the difficulty, and the next step was to support the case surrounding the suspended parts independently. A rough trial indicated some improvement, but at this point the experiments had to be laid aside for a time.

From the fact that the disturbance in question was produced by the slightest touch (as by a tap of the finger nail), upon the box, while the upper parts of the tube could be shaken with impunity, it appeared that it must depend upon a reaction between the air included in the box and the mirror. It is known that a flat body tends to set itself across the direction of any steady current of the fluid in which it is immersed, and we may fairly suppose that an effect of the same character will follow from an alternating current. At the moment of the tap upon the box the air inside is made to move past the mirror, and probably executes several vibrations. While these vibrations last, the mirror is subject to a twisting force tending to set it at right angles to the direction of vibration. The whole action being over in a time very small compared with that of the free vibrations of the magnet and mirror, the observed effect is as if an impulse had been given to the suspended parts.

In order to illustrate this effect I contrived the following experiment.* A small disk of paper, about the size of a sixpence, was hung by a fine silk fibre across the mouth of a resonator of pitch 128. When a sound of this pitch is excited in the neighbourhood, there is a powerful rush of air in and out of the resonator, and the disk sets itself promptly across the passage. A fork of pitch 128 may be held near the resonator, but it is better to use a second resonator at a little distance in order to avoid any possible disturbance due to the neighbourhood of the vibrating prongs. The experiment, though rather less striking, was also successful with forks and resonators of pitch 256.

It will be convenient here to describe the method adopted for regulating and determining the speed of rotation, which has proved thoroughly satisfactory. In the experiments of the Committee a governor was employed, and the speed was determined by means of the bell already referred to. This bell received a stroke every 100 revolutions, and the times were taken with a chronometer. In this method rather long spinnings (ten or twenty minutes) are necessary in order to get the speed with sufficient accuracy, much longer than are required to take the readings at the telescope. Desirous, if possible, of making the observations more quickly, I determined to try the stroboscopic method. On the axis of the instrument a stout card of 14 inches diameter was mounted, divided into concentric circles of black and white teeth. The black and white spaces were equal, and the black only were counted as teeth. There were five circles, containing 60, 32, 24, 20, 16 teeth respectively, the outside circle having the largest number of teeth.

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* "Proc. Camb. Phil. Soc.," Nov. 8, 1880.

This disk was observed from a distance through a telescope, and an arrangement for affording an intermittent view. An electric tuning fork of frequency about 63½ was maintained in regular vibration in the usual way by means of a Grove cell. To the ends of the prongs are attached thin plates of metal, perforated with somewhat narrow slits parallel to the prongs. In the position of equilibrium these slits overlap so as to allow an unobstructed view, but in other positions of the fork the disk cannot be seen. When the fork vibrates, the disk is seen intermittently 127 times a second; and if the speed be such that on any one of the circles 127 teeth a second pass a fixed pointer, that circle is seen as if it were at rest.

By means of the various circles it is possible to observe correspondingly varied speeds without any change in the frequency of the fork's vibration. A further step in this direction may be taken by modifying the arrangement for intermittent view. If the eye be placed at the top or bottom of one of the vibrating plates, a view is obtained once only, instead of twice, during each vibration of the fork. This plan was adopted for the slowest rotation, and allowed 60 teeth to take the place of 120, which would otherwise have been necessary.

The performance of the fork was very satisfactory. It would go for hours without the smallest attention, except an occasional renewal of the alcohol in the mercury cup. Pure (not methylated) alcohol was used for this purpose, and a *platinum* point made and broke the contacts. Although, as it turned out, this fork vibrated with great regularity, dependence was not placed upon it, but repeated comparisons by means of beats were made between it and a standard fork of Kœnig's construction, of pitch (about) 128. These beats, at pitch 128, were about 48 per minute, and scarcely varied perceptibly during the course of the experiments. They could have been counted for an even longer time, but this was not necessary. It was intended at first to make the comparisons of the fork simultaneous with the other observations, but this was given up as a needless refinement.

Some care was necessary in the optical arrangements to obviate undue fatigue of the eyes in a long series of observations. In daylight the illumination of the card was sufficient without special provision, but at night, when the actual observations were made, the image of an Argand gas flame was thrown upon the pointer and the part of the card near it. On account of the necessity of removing the electric fork and its appliances to a distance, the card, if looked at directly, would appear too much fore-shortened, and a looking-glass was therefore introduced. The eyepiece of the telescope, close in front of the slits, was adjusted to the exact height, and the eye was placed immediately behind the slits. By cutting off stray light as completely as possible, the observation may be made without fatigue and with slits narrow enough to give good definition when the speed is correct.

As governor I had originally intended to employ an electro-magnetic contrivance, invented a few years ago by La Cour and myself,* in which a revolving wheel is made to take its time from a vibrating fork, and it was partly for this reason that the water engine was placed at a considerable distance from the revolving coil. I was, however, not without hopes that a governor would be found unnecessary, and a few trials with the stroboscopic apparatus were very encouraging. It appeared that by having the water power a little in excess, the observer looking through the vibrating slits could easily control the speed by applying a slight friction to the cord connecting the engine and coil. For this purpose the cord was allowed to run lightly through the fingers, and after a little practice there was no difficulty in so regulating the speed that a tooth was never allowed finally to pass the pointer, however long the observation was continued. If, from a momentary inadvertence or from some slight disturbance, a tooth passed it could readily be brought back again. The power of control thus obtained will be appreciated when it is remembered that the passage of a tooth *per second* would correspond to less than one per cent. on the speed. In many of the observations the pointer covered the same tooth all the while, so that the introduction of a governor could only have done harm.

Another, and perhaps still more important, improvement on the original method related to the manner of making correction for the changes of declination which usually occur during the progress of the experiments. The Committee relied for this purpose upon comparisons with the photographic records made at Kew, and they recognise that considerable disturbances arose from the passage of steamers, &c. All difficulty of this kind is removed by the plan which we adopted of taking simultaneous readings of a second magnetometer, called the auxiliary magnetometer, placed at a sufficient distance from the revolving coil to be sensibly unaffected by it, but near enough to be similarly influenced by changes in the earth's magnetism, and by other disturbances having their origin at a moderate distance. The auxiliary magnetometer was of very simple construction, and was read with a telescope and a millimetre scale, the distance between mirror and scale (about $2\frac{1}{2}$ metres) being adjusted to approximate equality with that used for the principal magnet, so that disturbances were eliminated by simple comparisons of the scale readings. During a magnetic storm it was very interesting to watch the simultaneous movements of the magnets.

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* "Nature," May 23, 1878.

In the month of September the apparatus was remounted under the direction of Professor Stuart, to whose advice we have often been indebted. In order to examine whether any errors were caused by the circulation of currents in the frame, as has been suggested by more than one critic, insulating pieces were inserted, mercury cups at the same time being provided, so that the contacts could be restored at pleasure. But the principal changes related to the manner of suspending the fibre and supporting the box and tube. In order to eliminate tremor, as far as possible, these parts were supported by a massive wooden stand, resting on the floor and overhanging, but without contact, the top of the metal frame of the coil. The upper end of the fibre was fastened to a rod sliding in a metal cap, which formed the upper extremity of a 2-inch glass tube. Near the other end this tube was attached to a triangular piece of brass, resting on three screws, by which the whole could be raised or lowered bodily and levelled. Rigidly attached to this tube, and forming a continuation of it, a second glass tube, narrow enough to pass freely through the hollow axle of the coil, protected the fibre as far as the box in which the mirror and magnet were hung. This box was cylindrical and about 3 inches in diameter. The top fitted stiffly to the lower end of the narrow glass tube, and the body of the box could be unscrewed, so as to give access to the interior. The window necessary for observation of the mirror was made of a piece of worked glass, and was fitted air-tight.

On my return to Cambridge in October the apparatus was tested, but without the full success that had been hoped for. At high speeds there was still unsteadiness enough to preclude the use of these speeds for measurement. Since it is impossible to suppose that the tremor is propagated with sufficient intensity through the floor and massive brickwork on which the coil is supported, the cause must be looked for in the fanning action of the revolving coil, aggravated no doubt by the somewhat pendulous character of the box, and perhaps by the nearness of the approach between the coil and its frame at three points of the revolution.

At this time the experiment was in danger of languishing, as other occupations prevented Mr. Darwin from taking any further part; but on Dr. Schuster's return to Cambridge he offered his valuable assistance. Encouraged by Sir W. Thomson, we determined to proceed with the measurements, inasmuch as no disturbance, due to the rotation of the coil with circuit open, could be detected until higher speeds were approached than it was at all necessary to use.

One of the first points submitted to examination was the influence of currents induced in the frame. Without altering the speed or making any other change, readings were taken alternately with the contact-pieces in and out. Observations made on several days agreed in showing a small effect, due to the currents in the frame, in the direction of a *diminished* deflection. The whole deflection being 516 divisions of the scale, the mean diminution on making the top contacts was ·86 division. When the coil was at rest no difference in the zero could be detected on moving the contact-pieces.

In these preliminary experiments very consistent results were obtained at constant speeds, whether the rotation was in one direction or the other; but when deflections at various speeds were compared, we were startled to find the larger deflections falling very considerably short of proportionality to the speeds. There are only two corrections which tend to disturb this proportionality—(1) the correction for scale-reading, (2) the correction for self-induction. The effect of the first is to make the readings too high, and of the second to make the readings too low at the greater speeds. According to the figures given by the Committee (Report, p. 106), the aggregate effect is to increase the readings, on account of the preponderance of (1) over (2), whereas our results were consistently of the opposite character. Everything that could be thought of as a possible explanation was examined theoretically and experimentally, but without success. The coil was dismounted and the wire unwound, in order to see whether there was any false contact which might be supposed to vary with the speed and so account for the discrepancy. After much vexation and delay, it was discovered that the error was in the statement in the Report, the effect of self-induction being given at nearly ten times less than its real value. The correction for scale-reading, instead of preponderating over the correction for self-induction, is in reality quite a small part of the whole.

At this stage, as time was running short, we determined to proceed at once to a complete series of readings at sufficiently varied speeds, postponing the measurement of the coil to the end. The wire had been rewound without extreme care to secure the utmost attainable evenness, and the condition of the groove was such that a thoroughly satisfactory coil could not have been obtained, even with extreme care. It appeared, however, on examination that irregularities of this sort were not likely to affect the final result more than one or two parts in a thousand, if so much; and many points of interest could be decided altogether independently of this measurement.

The details of the experiments and reductions are given below by

Dr. Schuster, who took all the readings of the principal magnetometer. Mrs. Sidgwick observed the auxiliary magnetometer; while the regulation of the speed by stroboscopic observation fell to myself. Dr. Schuster also undertook the labour of the reductions and the final comparisons of our arbitrary German silver coil with the standard ohms.

The observations were very satisfactory, and at constant speeds agreed better than we had expected. The only irregularity that we met with was a slight disturbance of the zero, due to convection currents in the air surrounding the mirror, the effect of which, however, almost entirely disappears in the means. This disturbance could be magnified by bringing a paraffin lamp into the neighbourhood of the mirror. After about half a minute, apparently the time occupied in conduction through the box and in starting the current, the readings began to move off. Complete recovery would occupy twenty or thirty minutes. In future experiments this kind of disturbance will be very much reduced by increasing the moment of the magnet five or six times, and by diminishing the size of the mirror, both of which may be done without objection.

The comparison of the results at various speeds requires a knowledge of the cœfficient of self-induction L. Nothing is said in the Report as to the value of L for the second year's experiments, but the missing information is supplied in Maxwell's paper on the "Electro-magnetic Field,"* together with an indication of the process followed in calculating it. The first approximation to the value of L, in which the dimensions of the section are neglected in comparison with the radius of the coil, is 437,440 metres, but this is reduced by corrections to 430,165. The value which best satisfies the observations is considerably greater, viz., 456,748. A rough experiment with the electric balance gave 410,000; but Professor Maxwell remarks that the value calculated from the dimensions of the coil is probably much the more accurate, and was used in the actual reductions. I had supposed at one time that the discrepancy between the results at various speeds and the calculated value of L was due to the omission of the term in $\tan^4 \phi$, given above, which would have the same general effect as an under-estimate of L; but, as has been already mentioned, this term was in fact included in the reductions made by Mr. Hockin, in conjunction, moreover, with the value $L=437{,}440$.

A rough preliminary reduction of our observations showed at once that they could not be satisfied by any such value of L as 437,000, but pointed rather to 454,000, and we began to suspect that the influence of self-induction had been seriously under-estimated by the Committee. Preliminary trials by Maxwell's method with the electric balance giving promise of results trustworthy within one per cent., we proceeded to apply it with care to the determination of L, but the galvanometer at our command—a single needle Thomson of 2,000 ohms resistance—was not specially suitable for ballistic work. As this method is not explained in any of the usual text-books, it may be convenient here to give a statement of it.

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* "Phil. Trans.," 1865.

The arrangement is identical with that adopted to measure the resistance of the coil in the usual way by the bridge. If P be the resistance of the copper coil, Q, R, S, nearly inductionless resistances from resistance-boxes, balance is obtained at the galvanometer when $PS=QR$. This is a resistance balance, and to observe it the influence of induction must be eliminated by making the battery contact a second or two before making the galvanometer contact. Let us now suppose that P is altered to $P+\delta P$. The effect of this change would be annulled by the operation of an electromotive force in branch P of magnitude $\delta P . x$, where x denotes the magnitude of the current in this branch before the change. Since electromotive forces act independently, the effect upon the galvanometer of the change from P to $P+\delta P$ is the same as would be caused by $\delta P . x$ acting in branch P, if there be no E.M.F. in the battery branch at all.

Returning now to resistance P, let us make the galvanometer contact before making the battery contact. There is no permanent current through the galvanometer (G), but, at the moment of make, self-induction opposes an obstacle to the development of the current in P, which causes a transient current through G, showing itself by a throw of the needle. The integral magnitude of this opposing E.M.F. is simply Lx, and it produces the same effect upon G as if it acted by itself. We have now to compare the effects of a transient and of a permanent E.M.F. upon G. This is merely a question of galvanometry. If T be the time of half a complete vibration of the needle, θ the permanent deflection due to the steady E.M.F., α the throw due to the transient E.M.F., then the ratio of the electromotive forces, or of the currents, is

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\theta}.$$

If, instead of the permanent deflection θ, we observe the first throw (β) of the galvanometer needle, this becomes

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\frac{1}{2}\beta}.$$

In the present case, the ratio in question is, by what has been shown, above $\delta P . x : Lx$, or $\delta P : L$; so that

$$\frac{L}{P}=\frac{\delta P}{P}\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\frac{1}{2}\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9),$$

a formula which exhibits the time-constant of the coil P in terms of the period of the galvanometer needle. Further to deduce the value of L in absolute measure from the formula requires a knowledge of resistances in absolute measure.

In carrying out the experiment the principal difficulty arose from want of permanence of the resistance balance, due to changes of temperature in the copper coil. The error from this source was, however, diminished by protecting the coil with flannel, and was in great measure eliminated in the reductions. The result was L=455,000 metres. This is on the supposition that the ohm is correct. If, as we consider more probable, the ohm is one per cent. too small, the result would be L=450,000.

Without attributing too great importance to this determination, there were now three independent arguments pointing to the higher value of L: first, from the experiments of the Committee; secondly, and more distinctly, from our experiments; and thirdly, from the special determination; and I entertained little doubt that a direct calculation from the dimensions of the coil would lead to a similar conclusion.

This direct calculation proved no very easy matter. Mr. W. D. Niven (whom I was fortunately able to interest in the question) and myself had no difficulty in verifying independently the formulæ given in "Maxwell's Electricity and Magnetism," §§ 692, 705, from which the self-induction of a simple coil of rectangular section can be found, on the supposition that the dimensions of the sections are very small in comparison with the radius. In the notation of the paper on the electro-magnetic field, if r be the diagonal of the section, and θ the angle between it and the plane of the coil,

$$\mathrm{L}=4\pi n^2 a\left[\log_e\frac{8a}{r}+\tfrac{1}{12}-\tfrac{4}{3}(\theta-\tfrac{1}{4}\pi)\cot 2\theta-\tfrac{1}{3}\pi\operatorname{cosec}2\theta\right.$$
$$\left.-\tfrac{1}{6}\cot^2\theta\log_e\cos\theta-\tfrac{1}{6}\tan^2\theta\log_e\sin\theta\right]\quad . \quad . \quad . \quad (10).$$

In the paper itself, probably by a misprint, cos 2θ appears, instead of cosec 2θ, in (10). The expression is, as it evidently ought to be, unchanged when $\frac{1}{2}\pi-\theta$ is written for θ. By an ingenious process, explained in the paper, the formulæ is applied to calculate the self-induction of a double coil.*

The whole self-induction of the double coil is found by adding together twice the self-induction of each part and twice the mutual induction of the two parts. The self-induction of each part is found (to this approximation) by a simple application of (10). For twice this quantity Mr. Niven found 301,802, and I found 301,920 metres. For twice the mutual induction of the two parts I found, by Maxwell's method, 145,820 metres. Adding 301,920 and 145,820, we get 447,740 metres as the value of the whole self-induction, on the supposition that the curvature may be neglected. This corresponds to the value 437,440 given in the paper.

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* The following misprints may be noticed:—

Page 509, line 11, for B read C.
" " 13, for L(AC) read M(AC).
" " for L(B) read L(C).

Attention must be directed to the peculiar meaning attached to *depth*.

As to the origin of the discrepancy I am not able to offer any satisfactory explanation. It should be noticed, however, that owing to his peculiar use of the words "depth" and "breadth" as applied to coils, Maxwell has interchanged what, to avoid any possible ambiguity, I have called the *axial* and *radial* dimensions of the section. Thus the depth, *i.e.*, in his use of the word, the axial dimension, is given as ·01608, but this is really the radial dimension, as appears clearly enough from the Report of the Committee, as well as from our recent measurements. The real value of the axial dimension is ·01841 metre. But I do not think that this interchange will explain the difference in the results of the calculation.

When we proceed to apply corrections for the finite size of the section, further discrepancies develope themselves. The second term in the expression for L given in the paper (p. 508) does not appear to be correct, and the final numerical correction for curvature (−7,345 metres) differs in sign from that which we obtain. Mr. Niven has attacked the problem of determining the correction for curvature in the general case of a single coil of rectangular section, and (subject to a certain difficulty of interpretation) has obtained a solution. The application of the result to the actual case of a double coil would, however, be a very troublesome matter. For the two particular cases in which only one of the two dimensions of the section of a simple coil is considered to be finite, Mr. Niven and myself have independently obtained tolerably simple results. Thus, if the axial dimension be zero ($b=0$),

$$\mathrm{L}=4\pi n^2 a\left[\log\frac{8a}{c}-\tfrac{1}{2}+\frac{c^2}{96a^2}\left(\log\frac{8a}{c}+\tfrac{43}{12}\right)\right] \quad . \quad . \quad (11);$$

and if the radial dimension be zero,

$$\mathrm{L}=4\pi n^2 a\left[\log\frac{8a}{b}-\tfrac{1}{2}+\frac{b^2}{32a^2}\left(\log\frac{8a}{b}+\tfrac{1}{4}\right)\right] \quad . \quad . \quad . \quad (12).$$

Again, for a circular section of radius ρ,

$$\mathrm{L}=4\pi n^2 a\left[\log\frac{8a}{\rho}-\tfrac{7}{4}+\frac{\rho^2}{8a^2}\left(\log\frac{8a}{\rho}+\tfrac{1}{3}\right)\right] \quad . \quad . \quad . \quad (13).$$

In all these cases we see that the correction increases the value of L, and there can be no doubt that the same is true for the double coil.

I have applied (13) to estimate the correction for curvature in the self-induction of each part of the double coil. For reasons which it would take too long to explain, I arrived at the conclusion that the value of the small term must be very nearly the same for a circular section as for a square section of the same area, and the actual section is nearly enough square to allow of the use of this principle. The necessary addition to the originally calculated self-induction of each part, in order to take account of curvature, comes out 119·5 metres; so that the final value of L for the double coil will on this account be increased 239 metres. This is a small quantity, but a much larger correction for curvature must be expected in the mutual induction of the two parts. By a sufficiently approximate method I find as the correction to twice the mutual induction 3,469 metres, giving altogether for twice the mutual induction 149,289 metres. This added to 302,159 (=301,920+239) metres gives as the final calculated value of L for the double coil,

$$L=451{,}448 \text{ metres.}$$

This result is confirmed by calculation of the mutual induction by means of a table, founded on elliptic functions. In this way, and with a suitable formula for quadrature, we find,

$$2M=149{,}394 \text{ metres,}$$

agreeing nearly enough with the value found by Maxwell's method, viz., 149,289 metres.* When all the evidence is taken into consideration, there can remain, I suppose, little doubt that the value 451,000 is substantially correct, and that the reductions of the Committee are affected by a serious under-estimate.

Professor Rowland, in ignorance apparently of Maxwell's previous calculation, has shown that if in the original experiments we assume an unknown cause of error proportional to the square of the speed, and eliminate it, we shall arrive at a value of the ohm differing very appreciably from that adopted by the Committee. In this way he finds that—

$$1 \text{ ohm}=\cdot 9926\frac{\text{earth quadrant}}{\text{second}}.$$

Rowland is himself disposed to attribute the error to currents induced in the frame. Our experiments prove these currents had not much effect, though they may explain the difference between the value of L which best satisfies our experiments (where the currents could not exist), *i.e.*, 451,000, and the higher value 457,000 calculated by

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* The arithmetical calculations were made from the data given by the Committee (Reprint, p. 115), $a=\cdot158194$, $2b'=\cdot03851$, $b=\cdot01841$ (not ·1841), $c=\cdot01608$, all in metres. $n=313$. The whole number of turns (313) was supposed to be equally divided between the two parts.

Maxwell as most in harmony with the original experiments. The process adopted by Rowland is evidently equivalent to determining the coefficient of self-induction from the deflections themselves, and his result, rather than that given by the Committee, must be regarded as the one supported by the evidence of the original experiments.

Rowland's own determination, by a wholly distinct method, gives—

$$1 \text{ ohm} = \cdot 9911 \frac{\text{earth quadrant}}{\text{second}};$$

and according to our experiments the ohm is even smaller—

$$1 \text{ ohm} = \cdot 9893 \frac{\text{earth quadrant}}{\text{second}}.$$

The question, therefore, arises whether any further explanation can be given of the different result obtained by the Committee. The value of GK employed in calculating the experiments according to (4) was—

$$\text{GK} = 299{,}775 \text{ metres.}$$

For the principal term in GK, as given by (7), we require the values of n, a, and α. From p. 115 of the Reprint we find $a = \cdot 158194$ metre, $n = 313$. The angle α must be recalculated, as the value of $\log \sin^3 \alpha$ (1·9624955) is evidently incorrect. From $2b' = \cdot 03851$ metre by means of $\sin \alpha = a/\sqrt{(a^2 + b'^2)}$ we find $\log \sin^3 \alpha = \bar{1}\cdot 99043$. From these data the final value is—

$$\text{GK} = 299{,}290 \text{ metres,}$$

differing appreciably from that used by the Committee. The further discussion of the question is a matter of difficulty at this distance of time. There may have been some reason for the value adopted, which it is now impossible to trace, so that I desire to be understood as merely throwing out a suggestion with all reserve. But I think it right to point out a possible explanation, depending upon the interchange of the axial and radial dimensions in the paper on the electro-magnetic field. The data there given are the mean radius, the two dimensions of the sections, and the distance between the coils (·02010). This distance is correct, being equal to $2b' - b$, that is, to $\cdot 03851 - \cdot 01841$. The distance between the mean planes of the coils is not given, but could, of course, be calculated by addition of ·02010 and ·01841. If, however, the radial dimension ·01608 were substituted for the axial dimension ·01841, an erroneous value would be obtained for $2b'$, that is, ·03618 instead of ·03851. Using ·03618 to calculate α, I find—

$$\text{GK} = 299{,}860 \text{ metres,}$$

agreeing much more nearly with the value used in the reductions.

If it be thought probable that the value of GK was really 299,290, a still further reduction of nearly two parts in a thousand must be made in the number which expresses the ohm in absolute measure, and we should get—

$$1 \text{ ohm} = \cdot 9910 \frac{\text{earth quadrant}}{\text{second}},$$

coinciding practically with the value obtained by Rowland from his own experiments.

In the course of our experiments various doubts suggested themselves, and were subjected to examination. It may be well to say a few words about some of these, though the results are for the most part negative.

The energy of the currents circulating in the coil is expended in heating the copper, and a rise of temperature affects the resistance. Calculation shows that the disturbance from this cause is utterly insensible. If at the highest speeds of rotation all the heat were retained, the rise of temperature would be only at the rate of $3{\cdot}2 \times 10^{-8°}$ C. per second.

Much more heating may be looked for during the operation of taking the resistance. Under the actual circumstances a rise of resistance of about one part in 30,000 might be expected as the effect of the battery current in one minute. The aggregate duration of the battery contact in each of the resistance measurements was probably less than a minute.

Another question related to the possible effect of a want of rigidity in the magnetism of the needle. It is known that galvanometers will sometimes, when it is certain that there is no average current passing through the coils, show a powerful effect as a consequence of fluctuating magnetism corresponding to the fluctuating magnetic field. In the present experiment the magnetic field is fluctuating, and the magnet is expected to integrate the effect as if its own magnetism were constant. It is unlikely that any appreciable error arises in this way, as I find by calculation that a theoretically soft iron needle would point in the same direction as a theoretically hard needle when placed at the centre of the revolving coil.

From the details given the reader will be in a position to judge for himself as to the accuracy of our experiments. If, as we believe, the principal error to be feared is in the measurement of the coil, there is little to be gained by further experimenting with the present apparatus. Accordingly a new apparatus has been ordered, from which superior results may be expected. In designing this several questions presented themselves for solution.

All corrections being omitted, the effect—

$$\tan\phi \propto \frac{na^2\omega}{R};$$

and, if σ denote the section of the wire, and $S(=n\sigma)$, the aggregate section of the coil—

$$R \propto \frac{na}{\sigma} \propto \frac{n^2a}{S};$$

so that if S be given, tan ϕ is independent both of the number of terms n and of the mean radius a. If ϕ be given, the correction for self-induction depends upon L/GK, while both L and GK vary approximately as n^2a. So far, therefore, there is nothing to help us in determining n and a. The following considerations, however, tell in favour of a rather large radius :—

(1.) Easier measurement of coil.

(2.) Smaller correction for moment of suspended magnet.

(3.) Smaller errors from maladjustment to centre, and from size of magnet.

The question of insulation is important. During the rotations the electromotive force acts independently in every turn, and there is no strain upon the insulation; but in taking the resistance, when a battery is employed, the circumstances are materially different. Any leakage from one turn to another would, therefore, be a direct source of error. It is proposed to use triply covered wire.

In order to obtain room for the tube encasing the fibre, it is necessary to use a double coil. In the new apparatus there will be opportunity for a much larger diameter, by which it is hoped to obtain an advantage in respect of stiffness; but the further question presents itself, whether the interval between the coils should be increased so as to obtain a very uniform field, as in Helmholtz's arrangement of galvanometer. The advantages of this plan would be considerable in several respects, but on the whole I decided against it mainly on the ground that it would magnify the errors due to imperfect measurement. If we call the effect (so far as it depends upon the quantities now under consideration) u, we have, in previous notation,

$$u = a\sin^3\alpha = a^4(a^2+b'^2)^{-\frac{3}{2}},$$

$$\frac{du}{u} = \frac{4da}{a} - 3\frac{ada+b'db'}{a^2+b'^2}.$$

If $b'=0$,

$$\frac{du}{u} = \frac{da}{a},$$

but if, as in Helmholtz' arrangement, $b' = \frac{1}{2}a$,

$$\frac{du}{u} = \frac{8}{5}\frac{da}{a} - \frac{6}{5}\frac{db'}{a}.$$

The increase of b' from 0 to $\frac{1}{2}a$ not only introduces a new source of error in the measurement of b', but also magnifies the effect of an error in the measurement of a. If $b'=\frac{1}{10}a$, we have nearly

$$\frac{du}{u}=\frac{da}{a}-\frac{3}{10}\frac{db'}{a},$$

showing that an absolute error in b' has about $\frac{1}{3}$ of the importance of an equal absolute error in a.

As will be evident from what has been said already, the treatment of the correction for self-induction is a very important matter. It is probable that L may be best determined from the deflections themselves with the use of sufficiently varied speeds. If L be arrived at by calculation, or by independent experiments, it is important to keep down the amount of the correction. We have seen, however, that L/GK is almost independent of n, a, and S, so that if we regard $\tan\phi$ as given, the magnitude of the correction cannot be controlled so long as a single pair of coils is used. An improvement in this respect would result from the employment of two pairs of coils in perpendicular planes, giving two distinct and independent circuits. In virtue of the conjugate character, the currents in each double coil would be the same as if the other did not exist, and the effects of both would conspire in deflecting the suspended magnet. This doubled deflection would be obtained without increase of the correction for self-induction, such as would arise if the same deflection were arrived at by increasing the speed of rotation with a single pair of coils. A second advantage of this arrangement is to be found in the production of a field of force uniform with respect to time.

However the correction for self-induction be treated, it is important to obtain trustworthy observations at low speeds. In order to get a zero sufficiently independent of air currents, it will be advantageous largely to increase the moment of the suspended magnet. Preliminary experiments have, however, shown that there is some difficulty in getting the necessary moment in a very small space, in consequence of the interference with each other of neighbouring magnets, and thus the question presents itself as to the most advantageous arrangement for a compound magnet.

A sphere of steel, as used by the Committee, has the advantage that if uniformly magnetised it exercises the same action as an infinitely small magnet at its centre. But the weight of such a sphere is considerable in proportion to its moment, and it is probable that a combination of detached magnets is preferable. It is possible so to choose the proportions as to imitate pretty closely the action of an infinitely small magnet. Thus, if the magnet consist of a piece of sheet steel bent into a cylinder and uniformly magnetised parallel to the axis, the length of the cylinder should be to the diameter as $\sqrt{3}$ to $\sqrt{2}$. In this case the action is the same as of an infinitely small magnet as far as the fourth term inclusive of the harmonic expansion. Without loss of this property the cylinder may be replaced by four equal line magnets, coinciding with four symmetrically situated generating lines. Thus, if we make a compound magnet by placing four equal thin magnets along the parallel edges of a cube, the length of the magnets should be $\sqrt{3}$ times the side of the cube. This is on the supposition that the thin magnets are uniformly magnetised, as is never the case in practice. To allow for the distance between the poles and the ends of the bars, we may take the length of the bars 2·3 times the side of the cube.

With the new apparatus, and with the precautions pointed out by experience, we hope to arrive at very accurate results, competing on at least equal terms with those obtained by other methods. Most of the determinations hitherto made depend upon the use of a ballistic galvanometer, and the element of time is introduced as the time of swing of the galvanometer needle. There is no reason to doubt that very good results may be thus obtained, but it is, to say the least, satisfactory to have them confirmed by a method in which the element of time enters in a wholly different manner.

Part II.—By Arthur Schuster.

Adjustment of the Instruments and Determination of Constants.

The only adjustments to be made consist in—

1. The levelling of the coil.
2. The suspension of the magnet in the centre of the coil.
3. The proper disposition of the scale and telescope by means of which the angles of deflection are read off.

Level.—The first of these presents no difficulty, and any small error can be easily taken account of in the calculations. It was found that the upper end of the axis of rotation was inclined towards the north by an angle of ·0003 circular measure. Hence, as has already been explained (page 107) we must in the reductions write throughout $G\,(1+{\cdot}0003 \tan I)$ or $1{\cdot}0008\,G$ for G. This correction is small, but a little uncertain, as the coil was not very steadily fixed in its bearings, and small variations in the inclination of the axis could be produced by slightly pressing on one side or the other of the coil. When left to itself the coil seemed, however, very nearly to return to the same position.

The Magnet.—The magnet, which was suspended in the centre of the coil, consisted of four separate magnetised needles, each about 0·5 centim. long. These were mounted on four parallel edges of a small cube of cork. A needle attached to the back of the mirror went through a small hole in the cork, and was kept in its place by means of shellac, to prevent any slipping between the magnets and the mirror. The proper suspension of the magnet is a point of some delicacy and importance. As regards the vertical adjustment, the distance of the cube of magnets from the highest and lowest point of the circular frame was measured, and the magnet raised or lowered until the distances became equal. A pointer was next fixed to the frame, reaching very nearly to the centre of the coil. As the coil was rotated, the pointer described a small circle round the axis of revolution, and the position of the magnet could be easily altered until it occupied the centre of the small circle. It is supposed that this adjustment was made to within less than 1 millim., and could, therefore, for all practical purposes, be considered as perfect. The magnetic moment of the magnet was measured in the usual way. Two closely agreeing sets of measurement showed that at a distance of 1 foot it deflected a suspended needle through an angle, the tangent of which was ·000298. Hence at the mean distance of the coil (15·85 centims.) the deflection would have been ·0021. This number is equal to $\frac{\mathrm{MK}}{\mathrm{GH}}$, and will be referred to as $\tan \mu$ in the discussion of the calculations. The magnetic moment was determined a few days after the last spinnings had been taken; but on each day on which experiments were made, the time of vibration of the magnet was determined, and we thus assured ourselves that no appreciable change in the magnetic moment had taken place while the experiments were going on. The time of one complete vibration was 14·6 seconds.

Adjustment of Scale and Telescope.—The telescope which served to read the angle of deflection rested on a small table to which it could be clamped. In front of the table and below the telescope, the scale could be raised or lowered and fixed when the proper position had been found. It was levelled by deflecting the magnet successively towards both sides, and observing the point of the scale at which the cross wires of the telescope seemed to cut the scale. If in both positions of the mirror the scale was intersected at the same height, it was considered to be sufficiently levelled. It remained to place the scale at right angles to the line joining its centre to the mirror. This was done by measuring the distance of both ends to the mirror by means of a deal rod, with metallic adjustable pointers (presently to be described), and altering the position until these distances were equal. It is supposed that considerable accuracy was thus obtained. A small remaining error would be eliminated by observing deflections on both sides of the zero. To adjust the telescope we had now only to point it to the centre of the mirror, and at the same time to place it in such a position that its optic axis passed vertically over the centre of the scale. By suspending a plumb line from the telescope so as to divide its objective into two equal parts, and focussing alternately on the mirror and on the image of the scale, both points could be simultaneously attended to.

To measure the distance of the scale from the mirror the deal rod used for the adjustment of the scale was cut down so as to have nearly the required length. The two brass pointers attached to the two ends made an angle of about 45° with the rod. One of the pointers was fixed but the other could be moved round a fixed point in the rod by means of a screw. As it moved the distance of the two points changed, and by properly supporting the rod and leaning one point against the centre of the scale at a known height from the ground, while the moveable point was made to touch the centre of the mirror, the distance could be accurately found. It was compared with the scale itself, in order that the calculation of the angles of deflection should be independent of the absolute length of a scale division. The length required is the shortest line between the centre of the mirror and the plane of the scale, and this can be calculated if the difference in height of the centre of the mirror and the point to which the distance was measured, is known. These heights were determined by means of a cathetometer. The height of the centre of the objective was measured at the same time; so that all data required to find the inclination of the normal of the mirror to the horizontal are known. The following numbers were obtained; each division of the scale is for simplicity supposed to be equal to 1 millim., which is very nearly correct, but as has been said, its absolute value is of no importance.

Distance of mirror from scale in centims.	252·28
As the position of the magnet was always read off through a glass plate, a small correction equal to the thickness of the glass (3·2 millims.) multiplied into $1-\frac{1}{\mu}$, where μ is the refractive index, has to be applied. This correction is subtractive and equal to.	0·11
Hence, $D=$	252·17

It was also found that the mirror pointed downwards, and made an angle of ·004 with the horizontal. A small correction due to this cause will be discussed in another place.

Torsion.—The torsion was as much as possible taken out of the silk fibre, which was about 4 feet long, before the magnet was attached to the mirror. The coefficient of torsion was determined by turning the magnet through five whole revolutions and observing the displacement of the magnet. It was calculated from the numbers obtained that one revolution shifted the position of rest through 5·6 scale divisions, corresponding to an angle of ·001107.

Another experiment in which the magnet was turned in the opposite direction gave ·001117.

Hence $$\tau = \frac{\cdot 00111}{2\pi} = \cdot 00018.$$

The correction due to torsion is best applied to the value of G at the same time as the correction for level by writing everywhere—

$$G\,\frac{1+\beta \tan I}{1+\tau} \text{ for } G$$

Constants of the Coil.—The accurate determination of the constants of the coil forms the most difficult part of the measurements. Unwinding the coil, the outer circumference of the successive layers was measured by means of a steel tape. Each measurement was repeated several times, and the agreement was satisfactory. The thickness of the wire was found to be 1·37 millims., which, on the circumference of the successive layers, should produce a constant difference of 2·74 π or 8·62 millims. Owing, however, to defective winding, each layer enters a little into the grooves of the subjacent layer, and the differences in circumference of successive layers were therefore always smaller than they ought to have been. The differences varied between 7· millims. and 8·6 millims., but generally were about 8·1 millims. The wire was a little too thick, and as it had been taken off the coil and rewound, small irregularities were formed which prevented a satisfactory winding. Each coil had 156·5 windings. Of these 156 were in one coil regularly distributed over twelve layers of thirteen windings each; while half a turn was outside. In the second coil the twelve layers only contained 155 windings, and one turn and a half was lying outside. In the calculation for mean radius it was assumed that each complete layer contained the same number of turns. Let S be the sum of all measurement for one coil, also C the circumference of the layer containing the loose extra turns; then we find the mean circumference of the first coil, $\frac{13S+0\cdot 5\,C}{156\cdot 5}$ .... = 99·680

and for the second, $\frac{(13-1/12)S+1\cdot 5\,C}{156\cdot 5}$ .... = 99·651

Or as the circumference of the outside of the mean turn. = 99·666

From this is to be subtracted a correction equal π × thickness of tape .... = ·031

99·635

To obtain the circumference of the axis of the mean winding we have to subtract π × thickness of wire .... = ·431

Hence the final value of the mean circumference ...... β= 99 ·204
Or for the mean radius .... a= 15 ·789

The grooves of the coils and their distance was also measured, and it was found that—

b=axial dimension of coil.................... = 1 ·833
b'=distance of mean plane from axis of motion... = 1 ·918

All measurements are given in centimetres.

We calculate—

α=angle subtended at axis by mean radius $=\cot^{-1}\frac{o}{a}$... = 83° 04′

And $\log\sin^3\alpha$ .... = $\bar{1}$ ·990458
The principal term in the expansion of GK is $\pi n^2\beta\sin^3\alpha$=29,869,300
To this is to be added a small correction given on p. 107 = 100

The final value of GK being 29,869,200

Applying the corrections for level and torsion to GK as explained, and writing **𝔊𝔎** for the value so corrected, we find,

𝔊𝔎=29,887,600.

The Observations.

The observations consisted of two parts: the comparison of the resistance of the rotating copper coil with that of a standard German silver coil, and the observation of the deflections during the spinnings. The comparison of resistance was made by a resistance balance devised by Mr. Fleming,* to whom we are indebted for advice and assistance in all questions concerning the comparison of resistance. In this balance, which only differs by a more convenient arrangement from an ordinary Wheatstone's bridge, Professor Carey Foster's method of comparing resistance is used. The method consists in interchanging the resistance in the two arms of the balance which contain the graduated wire, and thus finding the difference between these two resistances in terms of that of a certain length of the bridge wire. The balance was placed on a table near the rotating coil, and could be electrically connected with it by means of two stout copper rods. The German silver coil which served as the standard of comparison was prepared so as to have a resistance nearly equal to that of the copper coil, that is about 4·6 ohms. Any error due to thermo-electric currents, which have sometimes been found to be generated at the moveable contact of the galvanometer circuit with the bridge wire, is eliminated in Foster's method, but to ensure greater accuracy and safety all measurements were repeated with reversed battery current. The whole comparison seldom occupied more than five minutes; and the readings obtained with the battery current in different directions closely agreed with each other.

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* "Phil. Mag.," ix, p. 109, 1880.

The spinnings were always taken in sets of four at the same speed, and the comparison of resistance was made at the beginning and end of each set. During the time of spinning the resistance was found to have altered owing to a rise of temperature which always took place during the time of experimentation. The corrections for the change of resistance and the possible errors introduced owing to the uncertainty of this correction will be described further on.

After the resistance of the coil had been measured, it was disconnected from the balance and set into rotation with open circuit, so that no current could pass. While the water supply was adjusted so as to give approximately the required speed, the magnet in the centre of the coil, which had been strongly disturbed during the measurement of resistance, was brought to rest either by means of an outside magnet or more often by means of a small coil and Le Clanché cell, which was always placed in the neighbourhood of the rotating coil. A key within reach of the observer served to make and break contact at the proper time. When the speed had been approximately regulated and the magnet was vibrating through a small arc only, its position of rest was determined, while at the same time the auxiliary magnetometer placed in the adjoining room was observed. The two ends of the rotating coil were now connected together, by means of a stout piece of copper, the well amalgamated ends of which were pressed into cups containing a little mercury, into which they tightly fitted.

As the coil was set into rotation the magnet slowly moved towards one side, and a proper use of the damping coil brought it quickly to approximate rest near its new position of equilibrium. When the swings extended through no more than ten or twenty divisions of the scale, the coil was kept, as nearly as possible, at the proper speed, by the observer at the tuning fork (Lord Rayleigh, see p. 112). Readings of the successive elongations were taken for about two minutes, and a signal given at the beginning and end of each set of readings enabled the observer at the auxiliary magnetometer (Mrs. Sidgwick) to note its position as well as any changes in the direction of the earth's magnetic force during the time of observation. The direction of rotation was now reversed, and the deflection observed in the same manner; the whole process being twice repeated, so that four sets of readings were obtained. When all the observations for the given speed had been completed, the position of rest of the magnet, when no current passed through the coil, was again determined and compared with the auxiliary magnetometer. A recomparison of resistance with the standard completed the set. The magnet in the centre of the coil should, when no current is passing through the coil, always go through the same changes as the magnet of the auxiliary magnetometer. If this could be insured, the two might be compared once for all, or the comparison might even be omitted altogether, for the difference between the deflections of positive and negative rotations, when corrected for changes in the earth's magnetism, would give the double deflection independently of the actual zero position. Unfortunately, however, and this was our greatest trouble, the comparison between the magnet and the auxiliary magnetometer showed that we had to deal with a disturbing cause, which rendered a frequent comparison between the two instruments necessary. This disturbing cause, which we traced to air currents circulating in the box containing the magnet, will be discussed presently.

The observations were taken on three different evenings and one afternoon. The evenings (8 h. P.M. to 11 h. P.M.) were chosen on account of the absence of disturbances, which, during the usual working hours, are almost unavoidable in a laboratory. We may give, as an example for the regularity with which the magnet vibrated round its position of rest, a set of readings which were taken while the coil revolved about four times in one second, the circuit being closed.

$T=9^{h}\ 36^{m}$. $t=13^{\circ}{\cdot}0$ C.

	Rotation.	Negative.
	374·4	362·1
	373·3	362·8
	372·2	362·0
	373·9	361·4
	372·8	362·0
	372·8	362·0
	372·4	363·8
	371·8	364·0
	371·1	364·0
	370·5	
Mean....	372·52	362·68

Position of rest, 367·60.

$T=9^{h}\ 38^{m}{\cdot}5$. $t=13^{\circ}{\cdot}0$ C.

The number of readings taken were not always the same, but varied generally between sixteen and twenty.

We used, in the course of our experiments, four different speeds. The method of obtaining and regulating these has been explained by

Lord Rayleigh. For simplicity we generally denoted the speed by means of the number of teeth on the circle which seemed stationary when looked at through the tuning fork; thus we spoke of a speed 24 teeth, 32 teeth, and 60 teeth. To obtain the lowest speed the circle containing 60 teeth was looked at over the top of the tuning fork, so that only one view for each complete vibration was obtained; this was equivalent to a circle of 120 teeth in the ordinary arrangement, which allowed a view for each half vibration, and, consequently, the lowest speed was called 120 teeth. The velocity of rotation depends, of course, on the frequency of the fork, which varied only within narrow limits, and was always very near 63·69. If f denote this frequency and N the number of teeth on the stationary card, the velocity of rotation ω is given by the equation $\omega=4\pi f/N$. In the "British Association Report" the speed is always indicated by means of the time occupied by 100 revolutions. If T is this time, we find $T=50N/f$. The following table gives the comparison of ω, T, and N, on the supposition that the frequency of the fork was always the same and equal to 63·69.

N.	ω.	T.	Number of turns per second.
120	6·670	94·206	1·06
60	13·339	47·103	2·12
32	25·011	25·122	3·98
24	33·348	18·841	5·31

The last column gives the number of turns per second.

Three speeds were taken on each of the three nights, and one set of observations with the lowest speed was secured in the course of one afternoon. We obtained in this way three sets for each of the two intermediate speeds and two sets for the lowest and highest speed. A comparison with the Report of the British Association Committee shows that we do not go up quite to their highest speeds, but that on the other hand our lowest speed was considerably below the one used by them. In the Report for the year 1863, it is mentioned that the forced vibration of the magnet, depending on the rotation of the coil, could not be noticed, and it is calculated that the amplitude of this vibration was less than $\frac{1}{100}$ of a millimetre on the scale. No mention is made of this forced vibration in the Report for 1864, although much lower speeds were used during that year. In our lowest speed a slight shake of the needle, due to the varying action of the currents in the coil, was distinctly seen; but as calculation showed that the amplitude was only the eighth part of a millimetre on the scale, no appreciable error is supposed to have been introduced by it. The moment of inertia of the suspended parts was higher in the experiments made by the British Association, and this, no doubt, is partly the reason why this forced vibration escaped their notice.

Air Currents.

It has already been noticed that air currents in the box containing the magnet affected its position to some extent, and we had to investigate in how far our final results might be affected by this disturbance. During the first night (December 2) our attention had not been drawn so much as it was afterwards to the effect of these air currents. We had previously ascertained, by a series of careful measurements, that the rotation of the coil with open circuit did not sensibly affect the zero position of the magnet, and we considered it sufficient to note the zero as short a time as possible before each set of four spinnings. The comparison of these zeros with the auxiliary magnetometer showed that during the two hours of experimenting, the needle had kept its zero within two divisions of the scale, so that the changes during two successive spinnings (generally about five minutes) must have been very small. On the second night (December 6) however, the zeros were taken at the beginning and end of each set of four spinnings, and the disturbance due to air currents was found to be of more importance. The following table reveals the fact that during a set of spinnings the magnet seems to have moved in one direction, but that during the time the coil was at rest and the comparison of resistance was made, it went in the opposite direction. The numbers given are corrected for changes in the direction of the earth's magnetic force as shown by the auxiliary magnetometer.

December 6.

Number of teeth on stationary circle.	Time. h. m.	Position of rest.	Approximate deflection.
60	8 53	763·60	218
	9 12	766·35	
32	9 31	764·88	397
	9 56	765·78	
24	10 9	762·67	514
	10 38	766·48	

Here, then, we have a gradual rise in the zero from one to over three divisions during a set of four spinnings. The approximate deflection is given in order to give an idea what amount of error the uncertainty of the zero might introduce.

Special experiments were now made, and it was found that by placing a lamp about a foot and a-half from the magnet box, changes amounting to eighteen divisions of the scale would be observed;

greater precautions were taken, in consequence of the experience thus gained, to secure the box from the radiation of the lamp and gas jets, which could not be dispensed with in the course of the experiments. The magnet box was covered with gold-leaf so as to reflect the heat as much as possible. On the last night of work frequent determinations of the position of rest were made, but in spite of all precautions an unknown cause produced a sudden displacement of five scale divisions. The exact time at which this change took place could not be determined, and two spinnings were therefore rejected. During the remainder of the evening the magnet gradually came back to its original position. With the exception of the two spinnings just mentioned we have not rejected any observations.

When we come to inquire into the amount of uncertainty to which our results are liable, owing to the effects of these air currents, we find that it cannot be greater than the more dangerous, because less evident, errors to which the determination of our constants (mean radius and distance of mirror from scale) are subject. As long as the changes of the position of rest take place irregularly, the error would tend to disappear in the mean, and the probable error deduced from our experiments would give a fair idea of the uncertainty due to this cause. This probable error, as we shall see, is very small. A regular displacement of zero in one direction would, however, produce a constant error which would not disappear in the final mean. We have some evidence that such a regular displacement has to some extent taken place. The comparison of zeros on December 6, as quoted above, for instance, shows the position of rest in the course of the spinnings shifted towards increasing numbers. Such a shift, if not taken into account, would tend to make the deflections towards increasing numbers (positive rotation) appear larger than those towards decreasing numbers. This, indeed, was observed. Supposing the shift takes place regularly during the time of spinnings we might have taken it into account. But the correction which we should have had to apply is so small and uncertain that it is doubtful whether we should have improved our final result, and it would certainly not have altered it within the limits within which we consider it accurate; for we find that reducing the deflections on the supposition, 1st, that the zero has kept constant; and 2nd, that it has changed uniformly during each set of spinnings; the two results agree to within about one and a-half tenths of a division, which, even at the lowest speeds, would only make a difference of about 1 in 750, and on the highest speeds four times less. The fact that a regular shift in the zero position of the magnet affects the mean of four spinnings is due to the arrangement of experiments, adopted during the first two nights, in which four rotations succeeded each other in alternate directions. If, after a rotation in the positive direction, two negative rotations, followed again by a positive one, had been taken, a regular displacement such as that we are discussing would not have affected the mean. This latter plan was adopted on the last night. In the measurements undertaken by the British Association Committee, the deflections in one direction were generally greater than in the other, and the difference was ascribed to a considerable torsion in the fibre of suspension, which, in order to explain the discrepancy, must, as pointed out by Rowland, have displaced the magnetic axis considerably out of the meridian. The differences in the readings taken when the coil was spinning in opposite directions were, on the average, about 3 per cent., and amounted in one case to 8 per cent. They show no regularity dependent on the speed of rotation. We also observed some slight differences of the same nature; but they are very small, and always remain within such limits that they may easily have been produced by a displacement due to air currents. On the last night, when more frequent zero readings were taken, the differences were, it is true, not much reduced in amount, but their sign was reversed. It is, perhaps, worth remarking that, owing to the absence of any controlling instrument equivalent to our auxiliary magnetometer, the Committee of the British Association had no opportunity of discovering the presence of these air currents, as any changes in the zero position would naturally have been ascribed by them to a casual change in the direction of the earth's magnetic force. Owing to the different shape and material of the box containing the mirror, it seems possible that the effect of air currents may have been considerably larger than it has been in our experiments.

Reduction of Observations.

Scale Corrections.—The first step in reducing the observations consists in calculating the value of $2 \tan \phi$ from the observed deflection. The correction to be applied to the reading in order to obtain numbers proportional to the tangents of deflection, if the position of rest of the magnet is at the centre of the scale, would be $-d^3/4D^3$; d being the observed reading, and D the distance of the mirror from the scale. If the zero, however, is at a point δ of the scale, the correction becomes $-(d-\delta)(d+\delta)^2/4D^2$, where δ is to be reckoned positive when in the same direction as d. For the higher speeds a second correction, to $+d^5/8D^4$, was applied, which comes just within the limits of accuracy aimed at in the actual readings. The corrected deflections so obtained are equal to $2D \tan \phi$. Small errors, due to a faulty division of the scale, ought also to be applied. It is difficult to obtain a proper scale in one piece of sufficient length to be used in these experiments; and the one in actual use consisted of three parts, cemented with caoutchouc cement to a thick piece of deal. No appreciable error was introduced by a very slight warping of the wood, and the scales were found to be very accurately divided, but the small errors existing were corrected; small corrections had also to be introduced, which are due to the imperfect joining of the different pieces. The combined correction never amounted to more than ·15 of a division. Each division, as has already been stated, being very nearly equal to 1 millim.

It has already been noticed that the normal to the mirror pointed slightly downwards. The correction due to this want of adjustment seems to have been generally neglected, yet it is not altogether unimportant. If p is the vertical distance between the centre of the objective and that point of the scale where it is cut by the normal to the mirror; also if α is the inclination between the normal to the mirror and the horizontal, the correction to be applied to a deflection d is $dp\alpha/D$, where D is the distance of the mirror from the scale. In our experiments the correction amounted to $d \times 0{\cdot}00014$, although the angle between the normal and the horizontal was only about 14 minutes of arc. The correction is positive only if the normal lies between the horizontal through the mirror and the line joining the mirror to the cross wires of the telescope.

Correction for Temperature.—We have now to discuss a series of corrections which have to be applied in order to make a comparison of the results obtained in different spinnings possible. It has already been noticed that four spinnings at the same speed were always taken into one set. The comparison of resistance at the beginning and end of each set showed that during the time of spinning the temperature had altered; before combining the mean within each set we had, therefore, to correct for the change of temperature. We endeavoured to keep the room as much as possible at a constant temperature during the experiments; the lamps used were always lighted nearly two hours before beginning, but, in spite of all precautions, the temperature always rose after we had entered the room and begun to work. The thermometer rose at first pretty rapidly through about 1 degree, and then rose slowly until at the end of the evening it stood generally nearly 2 degrees higher than at first. When the first set of spinnings commenced, the rapid rise, as shown by the thermometer in the room, had already subsided; but, as was to be expected, the temperature of the coil was lagging somewhat behind that of the room, and its resistance still rose appreciably. Thus, during the first night, the resistance of the copper coil rose almost ·4 per cent. during the course of the first set of four spinnings. If the curve of temperature of the coil is known, there is of course no difficulty in applying the proper correction. This curve can be obtained approximately by plotting down the measured resistances as ordinates with the time as abscissæ. This was done for all observations made on December 2; but during the succeeding nights it was found that the curve could not be sufficiently well determined by the observations, and that the assumption of a uniform rise in resistance during the time elapsing between two successive measurements would give as good results as the experiments themselves would allow us to obtain. The proper determination of this correction is a subject to which we shall have to give some attention in the more accurate measurements which we have in view. At present it will suffice to point out that, as far as we can judge, the error due to uncertainty of temperature is not more than ·05 per cent. during the first set of spinnings on each night. It is much smaller in the succeeding sets. It may increase the clearness of this account if at this point we give a specimen, worked out in detail, of one set of deflections. Let the resistance of our standard German silver coil, which we always have assumed to be at the temperature of the air, be called S, and the resistance of the rotating coil C. A comparison by means of the balance shows that

$$C=S+a,$$

where a is the resistance of a certain length of the bridge wire, differing slightly at the beginning and end of the experiment.

December 6.

Number of teeth on stationary circle, 32.

Comparison of resistance, C=S+·0225. Time=9^{h} 17^{m}.
Position of rest............ 766·48. Time=9^{h} 32^{m}.
Auxiliary magnetometer .... 26·9.

Time=	9^{h} 37^{m} ..	9^{h} 42^{m} ..	9^{h} 47^{m} ..	9^{h} 53^{m}
t=	13°·0 ..	13°·0 ..	13°·0 ..	13°·0
Rotation...............	negative..	positive ..	negative..	positive
Deflected reading........	367·60 ..	1166·40 ..	366·23 ..	1166·09
Auxiliary magnetometer..	27·55 ..	28·24 ..	28·50 ..	28·30

Auxiliary magnetometer .... 27·2. Time= 9^{h} 57^{m}.
Position of rest............ 767·08.
Comparison of resistance, C=S+·0272. Time=10^{h} 0^{m}.

From the comparison of zeros with the auxiliary magnetometer at the beginning and end of the experiments, we find for the corresponding readings during the experiments, 766·78 and 27·05. Considering that increased readings, if the magnet in the coil correspond to decreased readings in the auxiliary magnetometer, we find the following numbers for the positions of rest during the experiments :—

Position of rest.........	766·28	765·59	765·33	765·53
Deflected reading.......	367·60	1166·40	366·23	1166·09
Deflections	−398·61	+400·81	−399·10	+400·56
Scale correction	+ 2·08	− 2·93	+ 2·08	− 2·94
Reduction of temperature to Time=9^h 37^m	+ 0·05	+ 0·05	− 0·21	+ 0·35
Corrected deflection..	−396·55	+397·93	−397·23	+397·97

Mean deflection...... 397·42.

C=S+0·0248.

When all the spinnings had been reduced in this way, the final calculations for the actual resistance were made. The determination of all quantities involved has been explained, with the exception of the measurement of the absolute pitch of the tuning-fork.

Rate of Vibration of Tuning-fork.—As has already been explained, the tuning-fork which was used to regulate the speed was on every night compared with a standard fork, and our determinations, therefore, all depend on the absolute pitch of this standard fork. The method used to determine that pitch has been described by Lord Rayleigh.*

A fork, vibrating about 32 times a second, maintained by means of an electric current, and driving a second fork of fourfold frequency, was compared directly with the clock. The vibrations of the driven fork were simultaneously compared with the standard by counting the number of beats in a given time. A few experiments have to be made in order to see whether the fork gains on the clock, or *vice versâ*, and also whether the standard vibrates quicker or slower than the driven fork. This can be done by gradually shifting weights on the driver. The difference in the time of vibration of the clock and driving fork was generally such as to give one cycle in between 20 or 30 seconds. The driven fork gave at the same time from 5 to 11 beats per minute.

The experiments agreed well with each other, and both the rate of vibration and the temperature variation are in close agreement with the determinations made by Professor McLeod and Mr. G. S. Clarke† of other tuning-forks which, like ours, were made by König.

The following series of determinations was made at a temperature of about 13° C.:—

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* "Nature," xvii, p. 12, 1877.

† "Phil. Trans." vol. 171, p. 1, 1880.

128·179
·180
·181
·179
·174
·180
·189
·185

The small discrepancies would very likely be still further reduced if greater care was taken to ascertain the exact temperature of the fork. As a mean of different sets we find

Number of vibrations in 1 second$=128·180$ for $t=13°·0$ C.
$128·161$ $t=14°·6$ C.

From these data and the number of beats counted during each course of experiments we can, with the necessary accuracy, determine the number of vibrations of the fork, which served to regulate the velocity of the revolving coil.

Calculation of Results.—For accurate calculation, the expansion given in the Report of the British Association is not sufficient. Instead of taking into account a greater number of terms, we may with equal facility have recourse to the original quadratic equation for the resistance. We find

$$R=\frac{2f\mathfrak{GK}\cot\phi}{N}\left[\tfrac{1}{2}(1+\tan\mu\sec\phi)+\sqrt{\tfrac{1}{4}(1+\tan\mu\sec\phi)^2-U\tan^2\phi}\right].$$

In this equation, f, as before, is written for the frequency of the electrically maintained fork, and N for the number of the teeth on the apparently stationary circle.

U is written for $\frac{2L}{\mathfrak{GK}}\left(\frac{2L}{\mathfrak{GK}}-1\right)$.

The equation is correct if the torsion and deviation from level are taken into account in the value of GK as has been explained. The only approximation used in the equation is that of writing $\tan\mu$ for $\frac{KM}{GH}$.

Results.

The results of the calculation are collected in the following table. The first column contains the date on which the experiments were made; the second, the speed in terms of the number of teeth on the stationary card; the third column gives the deflection corrected for all scale errors and variations of temperature during each set; the fourth column shows the value of resistance in absolute measure as obtained by calculation on the assumption that the coefficient of self-induction of the coil is $4{\cdot}51 \times 10^7$ centims. This absolute resistance refers to the German silver coil, and a small length of the bridge wire at a given temperature. As both the temperature and this length of bridge wire varied in different experiments, the different results cannot be directly compared, but we can easily apply a correction which shall reduce the numbers to the absolute resistance of the German silver coil at a fixed temperature. The temperature chosen was 11°·5 C., which was approximately the lowest temperature observed in the course of the experiments. The fifth column contains the corrected values, which now can be compared together, and give the absolute resistance of the standard coil as observed on different occasions, and with different speeds. In the last column the mean value for the different speeds is given. In taking these, as well as the final mean, it must be observed that the set of observations made on December 10 with speed 60 teeth contained only two spins, or half the usual number.

Date.	Speed. No. of teeth on stationary card.	Deflection.	$R \times 10^{-9}$.	$R \times 10^{-9}$ corrected.	Mean.
Dec. 7...	120	110·42	4·5486	4·5419	4·5364
10...		110·22	4·5568	4·5309	
Dec. 2...	60	218·61	4·5580	4·5487	4·5467
6...		218·30	4·5620	4·5471	
10...		218·72	4·5531	4·5422	
Dec. 2...	32	397·75	4·5639	4·5417	4·5427
6...		397·39	4·5672	4·5415	
10...		397·26	4·5687	4·5448	
Dec. 2 ..	24	513·73	4·5719	4·5446	4·5442
6 ..		513·58	4·5734	4·5438	

The mean of all the observations is—

$$R = 4{\cdot}5427 \, \frac{\text{earth quadrant}}{\text{second}}.$$

The value of the self-induction which was adopted in these calculations is slightly smaller than the values calculated by Lord Rayleigh and Mr. Niven. A comparison of the results obtained with different speeds shows that the value must be very nearly correct, for there is no decided difference between the results. Nevertheless, it seemed of interest to calculate the value of the self-induction which best agreed with the experiments, and to see whether that value gave an appreciably different result for R.

We may, in fact, treat both R and L as unknown quantities, and employ the methods of least squares to find out the most probable values. We use for this purpose the approximate values already found, and find the most probable corrections to them. Neglecting the small corrections for torsion, magnetic moment, and level, and writing $P=2R/GK\omega$, we find for the quadratic which determines R—

$$P^2-P\cot\phi+U=0,$$

where U as before is written for $\frac{2L}{GK}\left(\frac{2L}{GK}-1\right)$.

We find approximately by differentiation, remembering that $dP/P=dR/R$,

$$d(\tan\phi)=-\frac{dR}{R}\left(\frac{1}{P}-\frac{3U}{P^3}\right)-\frac{dU}{P^3}.$$

We may consider dR/R and dU to be the unknown quantities to be determined. The coefficients with which they are multiplied are known with sufficient accuracy. $d\tan\phi$ is found for each observation by putting $dU=0$ and dR equal to the difference between the value of R calculated by means of that observation, and the value of R provisionally adopted. The usual methods to form the normal equations were employed. We find in this way—

$$R=10^9\times(4\cdot5433\pm0\cdot0019)$$

$$L=10^7\times(4\cdot5130\pm\cdot0110)$$

It is satisfactory to note that the final value of R derived with the aid of the theory of probability is practically identical with the mean value directly calculated from our experiments with $4\cdot51\times10^7$ as coefficient of self-induction. A remarkable agreement is shown between the value of this coefficient of self-induction best fitting our experiments................................ $4\cdot5130\times10^7$

and the value calculated from the dimensions of the coil .... $4\cdot5145\times10^7$

The large probable error, however, shows that the agreement is partly accidental.

To give an idea of the accuracy with which R has been determined by means of our experiments independently of constant errors, it may be mentioned that the probability of our value being wrong by one in a thousand is only one in ten, while the experiments made by the British Association give an even chance for the same deviation.

It only remains to determine the resistance of the German silver standard in ohms at a temperature of 11°·5 C.

We had at our disposal the original standards prepared by the Committee of the British Association. These are very nearly equal at the temperature at which they are supposed to be correct, and the ohm as determined by the Committee is, of course, uncertain within the limits within which the standards differ, but for our present purpose these may be considered identical. The coils were carefully compared by Mr. Fleming, who also determined their temperature coefficient. One coil in a flat case (hence called the "flat coil"), which had the smallest temperature coefficient, and supposed to be right at 14°·8 C., was taken at that temperature as the true ohm. Six of the standards were so arranged and joined together by means of mercury cups, that four were in a row, and the remaining two in double circuit, the whole system of coils being thus equivalent to about 4·5 ohms. Our standard German silver was nearly 4·6 ohms. As the difference was too great to allow a direct comparison by means of Mr. Fleming's bridge, a piece of German silver wire was prepared so as to have a resistance of ·1 ohm; this could easily be done within the required limits of accuracy by means of a set of resistance coils belonging to the Laboratory. Having thus a set of resistances very nearly equal to the one to be measured, a series of experiments was made on two successive days. Knowing all the temperature coefficients, we could easily reduce the measurements to ohms. Four different experiments gave for the German silver standard at 8°·5 C.—

4·5902
4·5896
4·5869
4·5879
4·5890

Mean = 4·5887

Assuming the German silver to vary 4·4 per cent. for 100° C., we find for our standard at 11°·5 C. 4·595 ohms. We have hitherto neglected to take account of the resistance of the two stout copper rods which connected the rotating coil with the resistance bridge. This resistance was determined by Mr. Fleming to be ·003 ohm. To make matters equal, we ought to have added that resistance to the British Association standards in comparing them with the standard used by us, and we should then have found that the absolute resistance found by us to be equal to $4{\cdot}543\,\frac{\text{earth quadrant}}{\text{second}}$, was equal to 4·592 ohms.

From this we calculate that the ohm as fixed by the Committee of the British Association is—

$$\cdot 9893\,\frac{\text{earth quadrant}}{\text{second}},$$

this being the final result of our experiments.

II. "On the Structure and Development of the Skull in Sturgeons (*Acipenser ruthenus* and *A. sturio*)." By W. K. Parker, F.R.S. Received April 14, 1881.

(Abstract.)

Since the publication of my paper on the skull of the salmon, a *Teleostean* fish, I have made many efforts to obtain early stages of the *Ganoid* types. Until the last two years, however, I have failed. At last, I have been taken almost by surprise by the kindness of Professor W. Salensky, of Kasan, and of Professor Alexander Agassiz, of Harvard College, Cambridge, Mass. The former has supplied Mr. F. M. Balfour and myself with a number of bottles of early stages of the sterlet (*Acipenser ruthenus*), whilst from Professor Agassiz we have received *fifty-four* bottles of fecundated eggs and embryos of various stages of the *Lepidosteus*.

Mr. Balfour's researches on the early stages of the sterlet, supplementing those of Professor Salensky, will appear immediately in the second volume of his work on "Comparative Embryology," and the result of his researches on the development of the *Lepidosteus*, in which he was assisted by one of my sons, will be sent to the Royal Society towards the fall of the present year. That paper, now far advanced, will be accompanied by one, already written and ready for publication, by me, on the development of the skull in the same species of Ganoid fish.

I had long ago received from the late Mr. Wm. Lloyd a number of young sturgeons of the common kind, but the *smallest* of them was 7 inches long; the largest of the larvæ of the small Russian species was only seven-twelfths of an inch in length.

Yet, notwithstanding the small size of these larvæ, their development had advanced very greatly and the cartilaginous framework was in a state of incipient completeness. Any little difficulty felt on stepping from the structure of specimens *seven lines* long to those *seven inches* long was done away with by the means at hand of comparison of the two types of Ganoids, namely *Acipenser* and *Lepidosteus*; for of the latter my specimens were very numerous and in every desirable stage.

Of the fry which Professor A. Agassiz and his talented assistant, Mr. S. Garman, had succeeded in rearing, many had reached to the length of an inch, and were pretty exact miniatures of their parents. But, besides these, I have received from Professor Burt Wilder, of Cornell University, Ithaca, Mass., U.S., some specimens of *Lepidosteus* ranging from 1 to 4½ inches in length.

We have thus been breaking ground, rather actively, among this most important and almost extinct "order" of fishes; a very important addition to the work will soon come to hand, namely, a memoir by Professor Traquair, on the adult skull of *Lepidosteus.*

I mention these details to show that biologists are working with a will, and working as yoke-fellows; and no doubt remains with me as to the result of such co-operation.

I must refer the reader to Professor Salensky's invaluable work on the development of the sterlet (Kasan, 1878), unfortunately published in *Russian*, and to the second volume of Mr. Balfour's work, for an account of the earliest stages of the *Acipenserine* embryo.

Even in larvæ one-third of an inch in length, the cartilage was becoming consolidated, and I was able to work out, by sections and dissections, the structure of the cranium and visceral arches; the one specimen which was seven-twelfths of an inch in length, and which was made into a large number of extremely thin sections, left nothing to be desired.

As I hope for an early publication of the present paper, I shall not enter very largely into details; they can only be followed, profitably, by help of the numerous illustrations.

The development of the skull of the sturgeon is very similar to what we find in the sharks and skates (" Selachians "), but the suspension of both the mandibular and the hyoid arches by one pier, derived from the hyoid (the *hyostylic* skull), which is seen in the Selachians on one hand and on the Holostean Ganoid and Teleosteans on the other, attains its fullest development in the "Acipenseridæ," or Chondrosteous Ganoids, for in them the "symplectic" is a separate cartilage, and not a mere osseous centre as in *Lepidosteus* and the Teleostei.

Here I find a very noticeable fact, namely, that whilst in the salmon the metamorphosis of the simple primary arches of the face can be followed step by step, in the sturgeon the peculiar modification of the arches shows itself during chondrification; the hyoid arch, from the first, is inordinately large.

Notwithstanding the huge size of the subdivided hyoid pier, its head only articulates in the larva with the auditory capsule; later on the basal cartilage reaches it, as in the Selachians.

But the arches that retain their normal size lend no colour to the theory that the visceral arches are related by their dorsal ends to the paired cartilages that invest the notochord, a state of things like that seen in the ribs and in the superficial cartilaginous hoops that surround the huge pharynx of the lamprey.

Mr. Balfour has demonstratively shown that in the branchial region, when the pleuro-peritoneal cavity has been subdivided by the hypoblastic outgrowths of the pharynx, the aortic arches lie *inside* the small temporary "head cavities," or remnants of the once continuous subdivision of the body wall into an inner layer, the "splanchnopleure," and an outer layer, the "somato-pleure."

But the aortic arches mount up, on each side, *outside* the proper branchial arches, which become grooved to receive them; these arches must, therefore, be considered as developments of the temporarily separate "splanchno-pleure"; they cannot be classed with the *costal* arches, which are developed in the permanently distinct "somato-pleure."

My dissections and sections, both of this type and of the Selachians, show, without leaving room for doubt, that all the visceral, or, properly speaking, *branchial* arches, mandibular, hyoid, and post-hyoid (branchial proper) are developed in the outer walls of the large respiratory pharynx, quite independently of the base of the skull and the fore part of the spinal column.

I have, at last, ceased to contend for true branchial or visceral arches in front of the mouth, and also to look upon the mouth and the openings around or in front of it as more than mere involutions of the epiblast; the first cleft is that between the mandible and the hyoid arch, the first arch is the mandibular.

With regard to the skull, it is now very evident that the "trabeculæ cranii," even in their furthest growth forwards from the end of the cephalic notochord, are merely foregrowths from the moieties of the investing mass (the parachordals), the true axis of the cranial skeleton ending under the fold of the mid-brain. The "cornua" of the trabeculæ, and the "intertrabecular" part or tract, are fresh shoots, so to speak, of cartilage that are specially developed to finish the cranial box and the internasal framework. I fear that my long-cherished pre-oral visceral arches will now have to go down and take their place among these secondary or adaptive growths.

I may remark, in concluding this very imperfect abstract, that the sturgeon is a very important type for the morphologist to get clear light upon.

In the Selachians the huge pterygoid foregrowth of the mandibular arch aborts the apex of its pier, the function of which is supplied by the hyo-mandibular; fragments only are developed in its upper part.

In the sharks from one to three mere "rays" are developed in front of the small upper remnant of the first cleft ("spiracle"); in skates there is, as a rule, a small separate piece, the true apex of the arch, its "pedicle." In one kind, however, the torpedo, four such fragments appear on each side, as shown by Gegenbaur. In the sturgeon there is a most remarkable plate in the common metapterygoid region, its form is rhomboidal; it is composed of a number of well-compacted pieces of cartilage, a middle series of *azygous* plates, and a somewhat irregular arrangement of plates right and left of these. This remarkable structure only exists in the Acipenseroids; it is not found in *Polyodon*.

In the Selachians the "placoid" plates or spines are not brought under the influence of the chondrocranium, which has neither parosteal plates applied as splints to it, nor ectosteal plates grafted upon it.

In *Acipenser* there are both parostoses applied to the oral apparatus, and ectosteal centres in the post-mandibular arches; moreover, along the side of the skull, in old individuals, plates of bone appear as these splints or parostoses, that are manifestly the forerunners of the deeper plates that, in the higher Ganoids and the Teleostei, form the proper ectosteal bony centres of the more or less ossified cranial box.

The Ganoid scutes of the sturgeon are so far dominated by the huge chondrocranium, that, by courtesy, they may be called frontal, parietal, opercular, and the like; of course, such scutes are not the accurate homologues of the bones so named in the Teleostei, which, at the most, can only correspond to the inner layer of the scute of such a fish as the sturgeon.

The sturgeons, as a group, cannot be said to lie directly between any one family of the Selachians and any one family of the Bony Ganoids, yet, on the whole, that is their position; the Bony Ganoids, on the whole, approach the Teleostei, especially such forms as *Lepidosteus* and *Amia*, which have lost their "spiracle," and in other things are less than typical, as Ganoids.

Larval sturgeons are, in appearance, miniature sharks; for a few weeks they have a similar mouth, and their lips and throat are beset with true teeth, that are moulted before calcification has fairly set in. Their first gills are very long and exposed, but not nearly so long, or for such a time uncovered, as in the embryos of sharks and skates.

III. "On the Estimation of the Amylolytic and Proteolytic Activity of Pancreatic Extracts." By WILLIAM ROBERTS, M.D., F.R.S., Physician to the Manchester Royal Infirmary and Professor of Clinical Medicine in Owens College. Received April 23, 1881.

The degree of activity possessed by preparations of the soluble ferments cannot be ascertained by direct weighing and measuring. The agents to which they owe their power have in no case been obtained in a state of isolation and purity. These agents are known to be indissolubly united with some form of albuminoid matter, and we are constrained to speak of them as if they really were albuminoid bodies. But their mode of action suggests an affinity with the imponderable forces, and points to the conclusion that the relation which these agents bear to their organic substratum is analogous, or at least comparable, to the relation subsisting between a mass of protoplasm and the vital endowments with which it stands possessed.

The activity of preparations of the soluble ferments can only be gauged by their capacity for work. But inasmuch as there is in them no power of growth and multiplication, the amount of energy with which they are endowed is strictly limited, so that when the capacity for work existing in a given liquid or solid preparation of one of these ferments has been ascertained, and has been put into due expression, the amount of energy in a certain quantity of the preparation can be counted in grams or cubic centimetres like that of any other chemical agent.

The term *ferment* has, up to this time, been applied in common to two groups of agents, which, although nearly allied both in their origin and in their mode of action, belong to essentially distinct categories. The organised or formed ferments, of which yeast is the type, are independent organisms with powers of growth and reproduction; and the transformations which constitute their special characteristics as ferments are inseparably associated with the nutritive operations of these organisms. The ferment-power cannot be separated from the ferment-organism by any method of filtration, nor by any solvent. The soluble ferments, on the other hand, pass freely into solution in water—their action is dissociated from the life of the gland-cells which produced them—and they are wholly devoid of the power of growth and reproduction.

Kühne has proposed to distinguish the group of soluble ferments by the name of "enzyms." I would suggest the desirability of adopting this term into English, with a slight change of orthography, as "enzymes," and also of coining from this root the cognate words which are requisite for clear and concise description. The action of an enzyme may be designated *enzymosis*, and the nature of the action may be spoken of as *enzymic*. In the present paper I shall venture to employ these terms in the sense here indicated.

The pancreas is known to be the source of two ferments or enzymes, of capital importance in the digestion of food, namely, an amylolytic enzyme, *pancreatic diastase*, and a proteolytic enzyme, *trypsin*. It is also known that the pancreas takes an important share in the digestion of fats, but it is doubtful whether its power in this respect is due to an enzyme or to an agency of a different character. The present paper concerns itself solely with the amylolytic and the proteolytic functions of the pancreas.

Estimation of the Amylolytic Activity of Pancreatic Extracts—Diastasimetry.

Probably the most accurate mode of estimating the activity of a diastasic solution is to ascertain the amount of sugar produced when a given quantity of the solution is made to act on a given volume of a standard starch mucilage, for a fixed time and at a fixed temperature. This method has already been recognised by Mr. H. T. Brown and Mr. J. Heron in their paper on the transformation of starch by malt infusions.* Kjeldahl has developed this method to a further point, and has used it to measure the comparative activity of malt infusions and of saliva.†

In the method about to be described, a simpler and speedier proceeding was employed, and the results were so brought out as to indicate absolute as well as comparative values. In principle the method consists in ascertaining the quantity of starch mucilage of known strength which can be transformed, by a unit measure of a diastasic solution, to the point at which it ceases to give a colour reaction with iodine, in a unit of time, and at a fixed temperature.

When starch mucilage is treated with extract of pancreas, or with any other diastasic solution, the mixture progressively loses its property of giving a colour reaction with iodine. First the blue reaction of unaltered starch passes away, then the brown and yellow reactions of dextrine successively disappear. It is not difficult to fix, with a fair amount of accuracy, the vanishing point of this reaction. This point may, for our present purpose, be called the *achromic point.*

The extract of pancreas employed in these observations was prepared by digesting fresh pancreas, freed from fat and chopped up, in four times it weight of dilute alcohol, containing 25 per cent. of rectified spirit (sp. gr. 0·838). The digestion was continued for four or five days, with occasional agitation. The mixture was then filtered through paper. Filtration is much facilitated by the addition to the solvent of 0·02 per cent. of acetic acid (containing 28 per cent. dry acetic acid).

The *standard starch mucilage* was made from potato starch. Owing to the large size of its granules, potato starch is easily obtained in a state of great purity, by repeated levigation with water, and afterwards drying the product at 40° C.‡ The mucilage was made of the strength of 1 per cent., and in the following manner: 5 grms. of starch were well stirred up into a thin mud with 30 cub. centims. of water, and then poured in a slender stream into 470 cub. centims. of briskly boiling water. The mixture was stirred and allowed to boil for a few seconds. Thus prepared the mucilage is perfectly smooth and uniform, and is so diffluent that it can be measured out like an ordinary liquid. This is known in the present paper as the *standard*

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* "Journal of the Chemical Society," September, 1879.

† "Compte Rendu des Travaux du Laboratoire de Carlsberg," 1879, p. 129.

‡ The so-called pure starch of the shops is worthless for the purposes of diastasimetry. A supply of pure potato starch may be obtained from Mottershead & Co., Chemists, Manchester.

starch mucilage. It should be used fresh, for it is apt to change in a few days, and to lose its opalescent appearance, and slight muculaginous consistency. When thus changed it is found to contain sugar. So long as it maintains its slight opalescence and slight mucilaginous character it is fit for use.

The *iodine solution* used in the testing was composed of 1 part of the Liquor Iodi of the British Pharmacopœia, diluted with 200 parts of water.

In determining the data on which is based the method of diastasimetry here proposed, it was necessary to ascertain the mutual relations in regard to the amylolytic process of three factors, namely, the *quantity* of pancreatic extract set in action, the *time* required to reach the achromic point, and the *temperature* at which the enzymosis was carried on.

Quantity and Time.—The amount of amylolytic work done by a given sample of pancreatic extract is strictly proportional to the quantity of it set in action—in other words, the amount of the standard starch mucilage which can be changed to the achromic point in a given time and at a given temperature, varies directly as the quantity of the extract employed. This law of proportionality may probably be regarded as fundamentally applicable to the action of all enzymes, which, having no power of growth or multiplication, conform in this respect to the common law which governs the action of ordinary chemical agents. The rule is, however, liable to interference if the products of the enzymosis accumulate in the solution to such a degree as to hamper the action. In the conditions observed in the following experiments this interference did not arise. The starch mucilage operated on was exceedingly dilute, and consequently the sugar and dextrines produced in the transformation never accumulated to such a degree as to check the enzymosis.

In the action of all enzymes the element of time is an essentially important factor. An enzyme liberates its energy gradually, in successive portions, and it takes a comparatively long time to exhaust itself completely. I found that pancreatic diastase, in the presence of excess of starch mucilage, took not less than forty-eight hours to completely exhaust itself at the temperature of 40° C.—("Lumleian Lectures" for 1880, "On the Digestive Ferments," p. 38).

The fundamental rule which governs the mutual relations of quantity and time in the action of an enzyme is that of *inverse proportion.* That is to say, that double the quantity of an enzyme will do a given amount of work in half the time, and that half the quantity will require double the time. This rule, however, is apparently controlled by another rule, namely, that *an enzyme liberates its energy at a progressively retarded rate.* If we conceive an enzyme as a body in a state of tension, charged with a certain amount of dormant energy, we can further conceive that in action it will discharge this energy gradually, and also at a rate which is continually diminishing. Such a conception will, I think, enable us to understand some features in the action of diastase and trypsin which are otherwise difficult to explain.

In regard to the action of pancreatic extract on starch mucilage, the rule of inverse proportion between quantity and time was found to hold good within considerable limits, as the following experiments show:—

Table I.

Experiments showing the inverse proportion between quantity and time in the action of pancreatic extract or starch mucilage. The quantity of the standard mucilage acted on in each experiment was 10 cub. centims. diluted with water up to 100 cub. centims. Temperature 15° C. The "calculated" time in the third column was obtained by taking the middle observation to each set as a standard of comparison.

	Quantity of pancreatic extract employed.	Time in which the achromic point was reached.	
		Found.	Calculated.
I.....	0·02 cub. centim.	34 minutes	36 minutes
	0·04 "	18 "	18 "
	0·08 "	9 "	9 "
	0·10 "	7 "	7⅕ "
	0·20 "	3 "	3½ "
II....	0·4 "	4¾ "	5 "
	0·2 "	10 "	10 "
	0·05 "	40 "	40 "

In both sets of observations the inverse time-rate is seen to come out true with almost mathematical accuracy.

When, however, a relatively small quantity of pancreatic extract was employed, and the time required to reach the achromic point was, in consequence, considerably lengthened, it was found that the advent of the achromic point was postponed beyond the term indicated by the rule. If the period occupied in reaching the achromic point fell within the compass of an hour, and the temperature was low, as in the observation just recorded, the inverse time-rate came out true, but when the period of action extended to several hours and the temperature stood higher, the departure from the rule was undoubted. The annexed table gives the results of an experiment made with a view of testing this point.

Table II.

Experiments showing the postponement of the achromic point when the action is protracted. The quantity of standard mucilage acted on in each experiment was 10 cub. centims. diluted with water up to 100 cub. centims. Temperature 40° C. The "calculated" time in the third column was obtained by taking the first observation, which was several times repeated, as a standard of comparison.

Quantity of pancreatic extract employed.	Time in which the achromic point was reached.	
	Found.	Calculated.
0·05 cub. centim.	10 minutes.	——
0·005 "	115 "	100 minutes.
0·004 "	140 "	125 "
0·002 "	300 "	250 "
0·0005 "	1,380 "	1,000 "

It need scarcely be said that when the enzymosis is very slow it is not possible to fix the vanishing point of the colour reaction with the same precision as when the action is more rapid and the change more abrupt. Notwithstanding this source of error, I think the conclusion indicated by the experiments may be relied on. The postponement of the achromic point shown in the table may be explained, as has been suggested, on the assumption that the enzyme liberates its energy at a continually retarded rate. In the case of trypsin, we shall see evidence of a precisely parallel phenomenon.

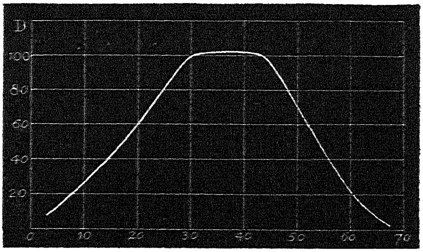
Temperature.—The action of pancreatic diastase on starch mucilage was found to increase in energy (or speed) from zero up to 30° C. From this point to 45° the rate of action continued steady, showing a range or platform of indifferent temperature extending from 30° to 45°. Above 45° the action became less and less energetic, and finally ceased between 65° and 70°. The following table exhibits the results obtained at various temperatures between 5° and 70°.

Table III.

Showing the effects of temperature on the action of pancreatic diastase; the amount of the standard mucilage acted on in each experiment was 10 cub. centims. diluted with water up to 100 cub. centims. The quantity of pancreatic extract employed in each experiment was 0·1 cub. centim.

Temperature.		Achromic point reached in
3—5° C.	....	36 minutes.
10	....	18 ,,
15	....	12 ,,
20	....	8 ,,
25	....	6 ,,
30	....	5 ,,
40	....	5 ,,
45	....	5 ,,
50	....	7 ,,
55	....	10 ,,
60	....	40 ,,
65	....	Very slow action.
70	....	No action.

These results, thrown into the form of a curve, are shown in the subjoined diagram. The ordinates indicate the diastasic value, or D, as calculated by a method to be presently explained; the abscissæ represent the temperatures.



Curve illustrating the effect of temperature on the action of pancreatic diastase.

Mode of proceeding.—In testing the activity of a sample of pancreatic extract, it was found on the whole more convenient to operate on a fixed quantity of the standard mucilage, and to vary the quantity of extract added to it, than to proceed contrariwise. The bulk of liquid operated on was thus kept constant. The ordinary proceeding was as follows: 10 cub. centims. of the standard mucilage were mixed in a beaker with 90 cub. centims. of water. The mixture was then warmed to 40° C., or at least to some point well within the range of indifferent temperature extending from 30° to 45°. This was done in order to eliminate the disturbing influence of temperature. The next step was to add the determined quantity of the extract to be tested to the diluted mucilage, and to note the exact time. Then, at short intervals, a drop of the enzymosing liquid was placed on a white slab, or plate, with a drop of the iodine solution. The time and result of each testing was noted. When the achromic point was reached the time was marked, and the interval from the commencement of the experiment was computed. If at the end of three minutes the mixture still gave the blue reaction of unaltered starch, a new experiment was made, using two, three, or four times the quantity of extract. If, on the other hand, the achromic point was reached in less than two minutes, a new experiment was made, using a smaller quantity of the extract. Two or three experiments generally sufficed to determine the quantity of extract required to bring the achromic point within a period ranging from two to ten minutes. A final control experiment enabled the operator to fix the achromic point somewhere between four and six minutes. The accuracy of the method depends chiefly on the sharpness and precision with which the occurrence of the achromic point can be determined. If it occur earlier than two minutes, the transition is too rapid for exact observation and record. On the other hand, if it occur later than fifteen or twenty minutes the transition is too gradual for precise limitation. The most satisfactory results are obtained when the achromic point falls between four and six minutes.

The following example will serve as an illustration of the way in which the experiments were carried out, noted, and expressed:—

Table IV.

10 cub. centims. standard starch mucilage + 90 cub. centims. water + 0·1 cub. centim. pancreatic extract—at 40° C.

Time.		Reaction with iodine.
10.30 A.M.	......	Commencement of experiment.
10.31 „	......	Blue.
10.32 „	......	Violet.
10.33 „	......	Brown.
10.34 „	......	Yellowish-brown.
10.35 „	......	Pale yellow.
10.36 „	......	No reaction—achromic point.
6 minutes.		

Achromic point reached in 6 minutes.

The result of the experiment was expressed in the first instance as follows: 0·1 cub. centim. pancreatic extract + 10 cub. centims. standard mucilage = 6 minutes at 40° C.

From this somewhat incongruous expression it is however easy to extract by a simple formula a correct and convenient expression for the diastasic value of any amylolytic solution.

Mode of Calculating and Expressing the Diastasic Value.

The principle of the method consists, as already stated, in ascertaining the amount of starch mucilage of known strength which can be transformed by a unit measure of the diastasic solution to the point at which it ceases to give a colour reaction with iodine, in a unit of time and at a given temperature.

In reducing this principle to a definite formula it was necessary to choose arbitrarily a unit of measure and a unit of time. The unit of measure fixed on was 1 cub. centim., and the unit of time 5 minutes. These selections seemed, on the whole, the best adapted for furnishing a convenient scale. On these bases the formula took the following form: the diastasic value of any solution—or D—is expressed by the number of cubic centimetres of the standard starch mucilage which can be transformed to the achromic point by 1 cub. centim. of the solution to be tested in a period of five minutes at a given temperature.

In the process of testing the quantity of the standard mucilage was made constant, namely 10 cub. centims., and the quantity of pancreatic extract and the time were made variable. In order to get the value of D the results must be so transformed as to make the quantity of extract and the time constant, and the quantity of the standard mucilage variable. This is accomplished by increasing or reducing the quantity of pancreatic extract employed to 1 cub. centim., and increasing or diminishing the standard mucilage in the same proportion. The product thus obtained is again increased or reduced in the same proportion as is requisite to increase or reduce the time found to five minutes. Taking the example above given, the value of D is obtained by the following formula: Let p signify the quantity of pancreatic extract employed, and m the number of minutes required to reach the achromic point, then:—

$$\frac{10}{p}\times\frac{5}{m}=\mathrm{D},$$

and in the above example—

$$\mathrm{D}=\frac{10}{0{\cdot}1}\times\frac{5}{6}=83 \text{ at } 40^\circ \text{ C.}$$

The value of D, as already explained, signifies the number of cubic centimetres of the standard starch mucilage which can be changed to the achromic point by 1 cub. centim. of the diastasic solution in five minutes at a given temperature. As the standard mucilage contains 1 per cent. of dry starch, the value of D divided by 100 gives us the same value in terms of dry starch, and the result of the above experiment may be read as follows:—

$$\mathrm{D}=83=0{\cdot}83 \text{ grm. of dry starch.}$$

This method of diastasimetry is equally applicable to saliva and malt-diastase. It may also be applied to the estimation of the diastasic agent which is present in urine, and presumably to all diastasic solutions. In the case of solid preparations containing diastase like malt or glandular tissue—a solution in known proportions must first be prepared; and from the ascertained activity of such solution the proportionate activity of the solid substance can be easily calculated. I may here mention some of the results which this method has already yielded.

Pancreatic Tissue.—The pancreatic tissue of the pig (obtained from animals killed for the market in the fasting state) yielded an extract which, when made on the large scale, possessed a mean diastasic value of 100. This extract is sent out by Mr. Benger, of the firm of Mottershead and Co., Chemists, Manchester, under the name of "Liquor Pancreaticus," and is made in the proportion of one part of pancreatic tissue to four of solvent (water containing 25 per cent. rectified spirit). This value indicates that 1 grm. of the moist pancreas of the pig is capable of transforming 4 grms. of dry starch to the point at which it no longer gives a colour reaction with iodine, in five minutes, at a temperature of 40° C.

The pancreatic tissue of the ox and sheep yielded an extract (made in the same proportions) which was of far inferior activity. The ox extract had a diatasic value of about 11 and that of the sheep of about 12. These numbers indicate that in point of diastasic activity the pancreas of the pig has ten times the value of the pancreas of the ox and sheep. This extraordinary difference is probably linked with the diversity of their food. The pig is fed largely upon potatoes and meal, which are rich in starch; the ox and sheep, on the other hand, feed on grass, which is poor in starch. We shall presently find that there is no such difference in regard to tryptic activity in the pancreas of these animals.

Human Saliva.—Filtered saliva was found to have a diastasic value varying from 10 to 17 at 40° C. Its action was influenced by temperature exactly in the same manner as that of pancreatic extract. It increased in energy up to about 30° C, continued steady from this point to about 45°, and then declined, being finally extinguished between 65° and 70°.

Malt Diastase.—Infusions of malt made in the proportion of one part of crushed malt to four parts of water, exhibited a diastasic value of 4 to 5 at 40° C. But malt diastase did not attain its maximum activity at this temperature. It continued to increase in energy up to about 60° C., when it showed a diastasic value of 10. Above 60° the action diminished in energy, but did not come to a full stop until the temperature approached 80° C.

Human Urine.—Several specimens of healthy urine were tested by this method. They showed a diastasic value varying from 0·03 to 0 13 at 40° C. The effect of temperature thereon was not examined.

Estimation of the Proteolytic Activity of Pancreatic Extracts—Trypsimetry.

The writer had found in previous inquiries that when milk is subjected to digestion with pancreatic extract, a striking change takes place in it at an early stage of the process. The milk acquires the property of curdling when boiled. The onset of this reaction occurs at an earlier or at a later period according to the activity of the extract and the quantity of it employed; and it is possible to fix the time of its advent with considerable accuracy—sufficient accuracy to serve as the basis of a method of measuring the proteolytic activity of pancreatic extracts.

The reaction in question depends on the production, as a first step in the pancreatic digestion of casein, of a modified form of that body which I have named *metacasein*. This substance resembles casein in being curdled by acetic acid in the cold; but it differs from casein in being also curdled by simple boiling. These two reactions together distinguish metacasein from other proteid bodies.

The property of curdling when boiled, which may be called the *metacasein reaction*, continues observable in milk undergoing tryptic digestion until near the termination of the process; it then disappears somewhat abruptly, and the milk, when boiled, remains fluid just as it did at first.

We may, therefore, speak of the *onset point* of the metacasein reaction, and of the *vanishing point* of the metacasein reaction. These two points mark respectively the initial and the terminal limits of the principal phase in the digestion of milk by pancreatic extract.

Before the onset point of the reaction—that is, distinct and undoubted curdling—is actually reached, its approach is indicated by an appearance of soiling of the sides of the test-tube in which the milk has been boiled. This appearance is due to incipient coagulation, which presently developes into pronounced curdling, and is a useful sign in testing to indicate the coming on of the metacasein reaction.

The following typical experiment may serve to give the reader a clear notion of the succession of events—so far as they concern us here—which occur when milk is submitted to digestion with pancreatic extract.

Table V.

4 cub. centims. pancreatic extract added to 50 cub. centims. milk diluted with water to 100 cub. centims. Temp. 18° C.

Time.	Reaction on boiling.
2 minutes ..	No change.
3 ,, ..	Slight soiling of the sides of the test-tube.
4 ,, ..	More soiling.
5 ,, ..	Distinct curdling—onset point of the metacasein reaction.

Time.	Reaction on boiling.
6 minutes ..	More pronounced curdling.
10 to 80 ,, ..	Pronounced curdling.
90 ,, ..	Diminished curdling.
95 ,, ..	Slight curdling.
100 ,, ..	No change; vanishing point of the metacasein reaction.

The length of time during which the successive steps of the transformation may continue observable depends on the energy of the action; and this, in its turn, depends on the activity of the preparation and the quantity of it added to the milk; it is also greatly influenced by temperature. By using an excess of an active pancreatic extract, and with a favourable temperature, all the steps of the process may be crowded almost into an instant of time; with converse conditions the action may linger on for many hours.

Although milk is a secretion of somewhat variable composition, the oscillations which it exhibits, when it is the product of a dairy, and is not intentionally adulterated, do not materially vitiate it for the purposes of a test fluid such as is here required. The milk delivered at my house presented very little variation. It had a density of 1030—seldom varying more than a degree from this point—and the results obtained with the milk of different days showed a remarkable uniformity. Milk from different dairies, and at different seasons of the year, would no doubt present greater irregularities. Milk should, however, be used fresh, for if it have become slightly acid, as it is apt to do in keeping, the results obtained are untrustworthy.

If milk be diluted with water the occurrence of the metacasein reaction is postponed; and the degree of postponement varies with the degree of dilution. For example, if 50 cub. centims of pure milk are changed to the onset point of the metacasein reaction in three minutes, the same quantity of milk diluted with an equal volume of water will take six minutes to reach the same point—other conditions being equal. There are, however, several advantages in using diluted milk instead of pure milk as the experimental fluid. The inequalities of the milk are thereby minimised. The "strike" of the reaction is more sharply defined, and the required quantity of pancreatic extract can be included in the water of dilution. This last is an important advantage, because if the extract to be tested is feeble, a considerable quantity of it requires to be added, and this, if pure milk were employed, would seriously alter its degree of dilution, and thereby vitiate the results. In the following experiments, milk diluted with an equal bulk of water was invariably employed; and if the quantity of pancreatic extract to be added exceeded 3 cub. centims. for every 50 cub. centims. of milk, this was always included in the water of dilution.

In principle the method of trypsimetry here proposed consists in ascertaining how many cubic centimetres of milk can be changed to the onset point of the metacasein reaction, in 5 minutes, by 1 cub. centim. of the extract to be tested, at a given temperature.

In settling the data on which the method is based, it was necessary, as in the case of diastase, to determine the relations to tryptic action of *quantity*, *time*, and *temperature*.

Quantity and Time.—The rule of inverse relations between quantity and time which was found to be valid within a wide range in the case of diastase and starch, is only reliable in the case of trypsin and milk within narrow limits. When the time of action exceeds 8 or 10 minutes the advent of the metacasein reaction is postponed beyond the term indicated by the rule of inverse proportion, and this postponement increases as the time of action is lengthened. The following two sets of observations may be taken as samples of the results obtained by experiment.

Table VI.

Showing the postponement of the metacasein reaction.

The quantity of milk acted on in each experiment was 50 cub. centims. diluted with water up to 100 cub. centims. The "calculated" time in the third column was obtained by taking the first observation in each set as the standard of comparison.

Quantity of pancreatic extract added.	Onset point of the metacasein reaction.	
	Found.	Calculated.
Set I.—Temperature 40° C.		
1·0 cub. centim.	3 minutes.	——
0·8 "	4 "	3¾ minutes.
0·6 "	5 "	5 "
0·4 "	9 "	7½ "
0·2 "	30 "	15 "
Set II.—Temperature 16° C.		
4·0 cub. centims.	6 minutes.	——
2·0 "	16 "	12 minutes.
1·0 "	39 "	24 "
0·5 "	105 "	48 "
0·25 "	280 "	96 "

When the vanishing point of the metacasein reaction was taken as the point of comparison, the results approximated more nearly to the requirements of the rule of inverse proportion, especially at low temperatures; but still the evidence pointed in the same direction, and indicated that trypsin, like diastase, exhausts itself in action at a progressively retarded rate. From the numerous experiments which were performed with a view of elucidating this point, I arrived at the conclusion that when the onset point of the metacasein reaction fell between 3 and 6 minutes the inverse time-rate gave a reliable basis of calculation, but not beyond these limits.

Temperature.—Tryptic enzymosis is exceedingly sensitive to temperature. The action of trypsin on milk increases in energy from zero to 60° C. Above this point there is a rapid fall, and the action is finally arrested between 75° and 80° C. There is not, as with diastase, any range or platform of indifferent temperature. The following table exhibits the degrees of activity from 10° to 80°. In order to obtain the utmost uniformity of results, the quantities of pancreatic extract employed were so adjusted as to bring the incidence of the metacasein reaction within a period ranging from 4 to 6 minutes.

Table VII.

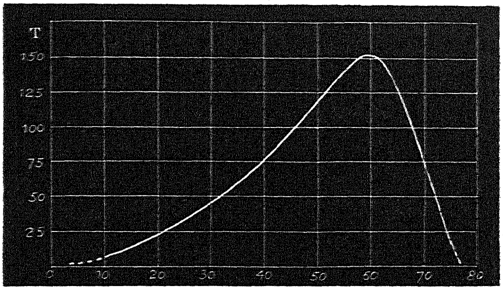
Showing the effects of temperature on tryptic enzymosis.

The quantity of milk employed in each experiment was 50 cub. centims. diluted with water up to 100 cub. centims. In the fourth column the degree of tryptic activity, or T, is calculated by a method to be presently explained.

Temperature.	Quantity of pancreatic extract employed.	Onset point of the metacasein reaction.	Tryptic value—or T.
10° C.	6·0 cub. centims.	5 minutes.	8
15	4·0 „	5 „	12
20	3·0 „	4 „	21
30	1·0 „	5½ „	45
40	0·6 „	5½ „	76
50	0·4 „	5½ „	119
60	0·3 „	5½ „	150
65	0·4 „	5 „	125
70	0·8 „	4 „	78
75	2·0 „	6 „	21
80	4·0 „	No action.	0

In the subjoined diagram these results are thrown into the form of a curve. The ordinates indicate the degrees of tryptic activity (or T), and the abscissæ indicate the temperatures.

In another series of experiments the effect of temperature was gauged by the length of time required to reach the onset or the vanishing point of the metacasein reaction when constant quantities of pancreatic extract were used. The results obtained in this series are tabulated in Table VIII. In the first set the onset point of the reaction was taken as the index of tryptic activity; in the second set



Curve illustrating the effect of temperature on the tryptic digestion of milk.

the vanishing point of the reaction was employed for the same purpose. The results brought out by these experiments correspond pretty closely with those given in Table VII.

Table VIII.

Showing the effect of temperature by the length of time required to reach the metacasein reaction, when constant quantities of pancreatic extract are used.

I Set. 0·4 cub. centim. pancreatic extract with 100 cub. centims. diluted milk.		II Set. 4 cub. centims. of pancreatic extract with 100 cub. centims. diluted milk.	
Temperature.	Onset point of the metacasein reaction.	Temperature.	Vanishing point of the metacasein reaction.
2 to 5° C.	312 minutes.	—	—
10	168 „	10° C.	180 minutes.
15	120 „	—	—
20	70 „	20	75 „
30	25 „	30	26 „
40	12 „	40	12 „
50	6 „	50	6 „
60	4 „	60	4 „
65	6 „		
70	action suspended, but resumed on cooling.		

An examination of the table shows how very nearly the results correspond, whether the onset point or the vanishing point of the metacasein reaction be taken as a measure of tryptic activity. This correspondence substantiates the conclusion that the onset point of the reaction furnishes a trustworthy index of the activity of tryptic digestion. The proportionate quantity of pancreatic extract added to the milk in the experiments recorded in Set II of Table VIII was ten times as great as in those recorded in Set I; and it is seen that, by using these proportions, the vanishing points and the onset points fell out in nearly the same times in both sets of experiments.

Mode of Proceeding.—In testing the tryptic activity of a sample of pancreatic extract, the following procedure was adopted:—50 cub. centims. of fresh milk were diluted with 50 cub. centims. of water, less the quantity of extract intended to be added. The diluted milk was then warmed to 40° C., and maintained exactly at that temperature until the close of the experiment. The intended quantity of the pancreatic extract, say 1 cub. centim., was then added, and the time exactly noted. At the end of each minute a portion of the digesting milk was withdrawn, and boiled for a few seconds in a test-tube, inclining the test-tube to one side after the boiling in order to observe the effect. The result was at once noted down. As soon as distinct curdling occurred on boiling, the experiment was considered finished; the time was recorded, and the number of minutes which had elapsed from the commencement of the experiment were reckoned. The result came out in the following form:—

1 cub. centim. panc. extract+50 milk=4 minutes at 40° C.

If no signs of incipient curdling (soiling of the sides of the test-tube) occurred within 3 minutes, a new experiment was made, using, two, three, or four times as much pancreatic extract. If, on the other hand, distinct curdling occurred in 2 minutes, or less, a fresh experiment was made, using half or quarter the quantity of extract. Three or four such experiments usually sufficed to enable the operator to fix the onset point of the reaction somewhere between 4 and 6 minutes.

Mode of Calculating and Expressing the Tryptic Value.—The object of the experiment was to ascertain how many cubic centimetres of milk can be changed to the onset point of the metacasein reaction by 1 cub. centim. of extract in a period of 5 minutes, at the temperature of 40° C. The tryptic value, or T, was calculated from the first expression of the results of an experiment in exactly the same way as for diastase. If p be made to signify the quantity of pancreatic extract added to the milk, and m the number of minutes which were required to reach the onset point of the metacasein reaction, then the value of T was obtained by the following formula:—

$$\frac{50}{p}\times\frac{5}{m}=\mathrm{T},$$

and taking the experiment above given the value of T came out as follows:—

$$\mathrm{T}=\frac{50}{1}\times\frac{5}{4}=62{\cdot}5 \text{ at } 40° \text{ C.}$$

In judging the practical value of this method of trypsimetry, one must have regard to the inherent difficulty of estimating the activity of preparations of the proteolytic enzymes. I venture to think that we have in this method a means of estimating the activity of trypsin preparations which is superior in ease and precision to any we possess for the evaluation of pepsin preparations. What may be the limits of error arising from inequalities in the composition of milk I am unable to say, but with the same specimen of milk the limits of error do not certainly exceed 6 to 8 per cent.

The tryptic value of pancreatic extract from the pig, made on the large scale, was found to range from 40 to 70 at 40° C. The pancreatic tissue of the ox and sheep yielded an extract which possessed about the same tryptic activity as that of the pig. Extracts prepared from single glands presented very considerable variations both in regard to their diastasic and their tryptic activity. The following table shows the enzymic values of twelve samples of pancreatic extract prepared with single glands from four pigs, four oxen, and four sheep, killed for the market.

Table IX.

(All the observations were made at 40° C.)

(D stands for diastasic value, and T for tryptic value.)

Pig.	Ox.	Sheep.
No. 1 D=166 T=64	No. 5 D=8 T=64	No. 9 D=5 T=125
No. 2 D=100 T=83	No. 6 D=10 T=50	No. 10 D=12 T=83
No. 3 D=100 T=72	No. 7 D=9 T=42	No. 11 D=14 T=64
No. 4 D=100 T=64	No. 8 D=13 T=83	No. 12 D=4 T=28

It may be observed that the oscillations in the two enzymic values bear no mutual relations to one another.

The most appropriate standard of temperature for the valuation of tryptic activity, is 40° C., because this corresponds very nearly with the temperature at which trypsin operates in the normal digestion of warm-blooded animals. But it is more convenient to perform the testing at, or near, the ordinary temperature of the room (say, at 20°) inasmuch as in the latter case, it is much less troublesome to maintain a continuously uniform temperature than at 40°.

I have, therefore, taken some pains to ascertain the exact relation between the value of T at 40° and at 20° respectively, and have found that at 40° the value of T is very nearly three and a half times as great as at 20°. If, therefore, the testing be performed at 20°, the resulting value of T multiplied by 3·5, will give with sufficient accuracy the value of T at 40°.

May 12, 1881.

GENERAL STRACHEY, R.E., C.S.I., V.P., in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Pursuant to notice Gabriel Auguste Daubrée, Jean Charles Marignac, Carl Nägeli, and Carl Weierstrass were balloted for and elected Foreign Members of the Society.

The following Papers were read :—

I. "On the Physiological Action of β Lutidine." By C. GREVILLE WILLIAMS, F.R.S., and W. H. WATERS, B.A., Demonstrator in the Physiological Laboratory, Cambridge. Received April 23, 1881.

In studying the physiological action of β lutidine the greater number of our experiments were made upon the frog, and more particularly related to the action of this substance upon the *heart* and *central nervous* system; our other experiments upon the different organs being for the better interpretation of the action upon these two.

Heart.

In all cases we first destroyed the brain and spinal cord of the frog by pithing, and with the smallest possible loss of blood. Hence none of the results obtained could be due to the central nervous system.

The experiments were made upon the heart to study the effect of the β lutidine with regard to its—

(1.) Tonicity;
(2.) Time of beat;
(3.) Nerve supply.

It may be stated at once, that the pure alkaloid applied to the surface of the heart causes that organ at once to shrink up, turn pale, and cease to beat. At times it was found possible to restore these beats by washing with normal saline solution (0·75 per cent. sodium chloride), but in no case did they ever reach their former activity.

(1.) *Tonicity of the Heart.*—We made use of several methods during our experiments to ascertain the effect of this poison upon the heart's tonicity.

The three chief ones were—

(*a*.) Moist chamber and lever;

(*β*.) Roy's apparatus;

(*γ*.) Gaskell's method of the excised heart.

(*a*.) *Moist Chamber and Lever.*—In this method the frog had just sufficient of its skin and sternal cartilage removed to expose the apex of the heart; a carefully balanced lever (of the first kind) had attached to one end of it, by a horizontal pivot, a light needle, which, by means of a small piece of cork, just rested upon the apex of the heart. The other end of the lever carried a fine piece of aluminium to mark the beats upon the smoked surface of a revolving cylinder. Another piece of aluminium recorded the abscissa line from which variations in the height of the curve of ventricular beats could be recorded. A glass shade, having a slit to allow the end of the lever to protrude and work upon the drum, was then placed over the animal, and the air inside kept moist with damp blotting-paper.

From the arrangement of the apparatus it is evident that at each ventricular systole the marker would fall, and at each diastole rise; any increase in the tonicity of the heart being indicated by the tracing approaching the abscissa line.

A tracing of the heart's normal beat was in all cases first taken, and then various quantities of various strengths of β lutidine were injected into different parts of the body, from a drop or two of 2 per cent. solution in the lymph spaces near the heart, to 0·2 cub. centim. of 2 per cent. in the lower part of the abdominal cavity. In all cases there was a very evident lowering of the lever's marking point and a decrease in the size of the beats, clearly showing that the heart had experienced a distinct increase in its tonicity. In all cases it became much paler.

No. I is a characteristic series of tracings showing the alterations in the ventricular beats. Of these (1) is that made by the normal heart, while 2, 3, and 4, were taken at intervals of one, three, and six minutes after the injection of 0·2 cub. centim. of 2 per cent. β lutidine into the abdominal cavity. The increase in tonicity and slowing of the beat are most evident. These tracings should be read from right to left, *a a* being the abscissa line.

(*β*.) *Roy's Apparatus.*—In this apparatus the heart is tied upon a double canula, one limb of which is in connexion with a supply of fresh diluted sheep's blood (one part of defibrinated blood to three parts of 0·75 per cent. saline solution), kept at a constant height in a reservoir; the other allowing the blood as it is pumped off by the heart to flow away. The surface of the fluid in the reservoir being always at the same height, variations in the heart's beat could in no way be due to variations in pressure of the fluid. The heart tied to the canula is inserted into a closed chamber filled with oil. By a particular arrangement, a piston, working in a cylindrical prolongation of this chamber, will rise or fall on each systole or diastole of the ventricle. A lever being attached to this piston indicates the condition of the ventricle upon a moving surface.

From the construction it is evident that each systole of the ventricle is indicated by a rise in the lever, making evident its character.

After the character of the beats of a particular heart had been observed while the normal blood was passing, the blood was then sent through, containing from 0·1 per cent. to 0·4 per cent. of β lutidine, and the character of the tracings studied. These were distinctly modified; the heart still beat, but never relaxed during diastole to its full extent, in fact, showing a most decided increase in its tonicity. The highest portions of the curve, indicating the condition of the ventricle during systole, showed that that condition was much prolonged, that portion of the curve being much rounder and longer. The normal beat could be at once fully restored by repassing the saline blood solution; and a series of experiments, lasting some time, could thus be taken.

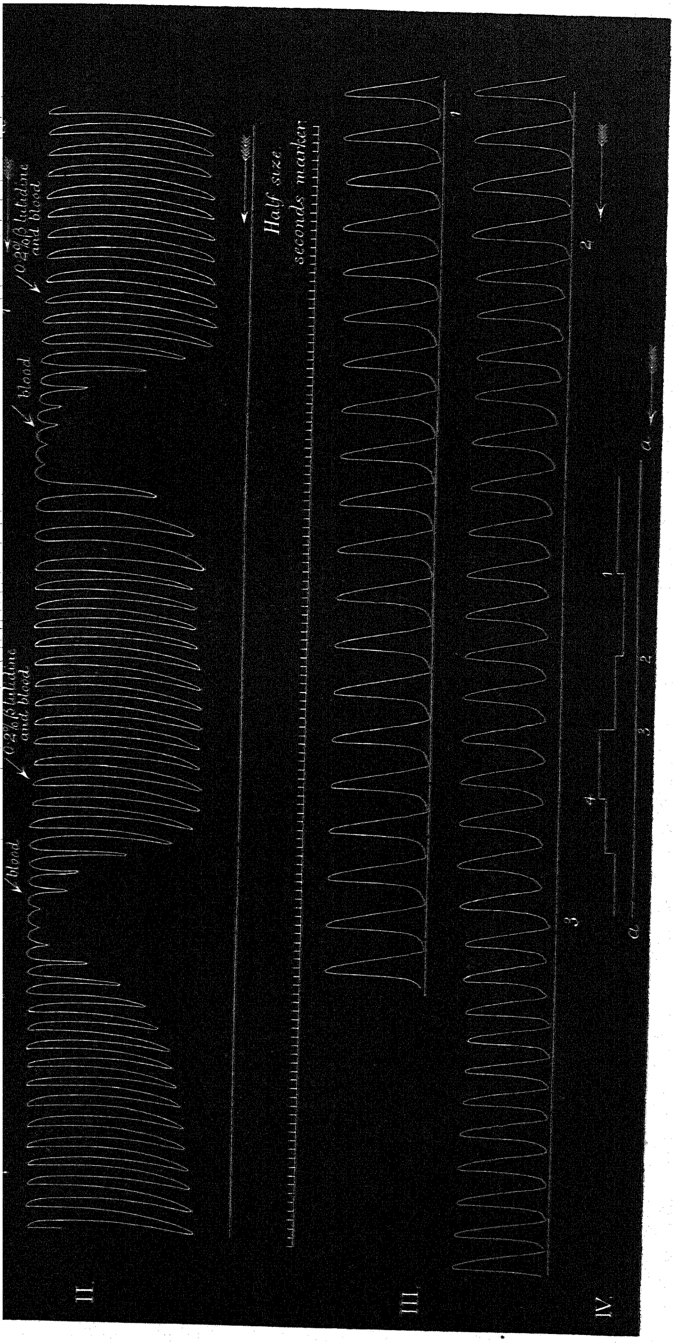
II is a specimen of these tracings, and should be read from right to left. The beats on the extreme right are those of the ventricle before the application of the alkaloid. When weaker solutions are used, similar alterations occur, though naturally not so pronounced.

In one experiment, where no beats occurred, the lever rose distinctly on passage of the saline blood containing the β lutidine, indicating an increase in the tonicity of the ventricle.

(γ.) *Gaskell's Method.*—In these experiments a method due to Dr. Gaskell was used. A piece of fine silk was tied to the apex of the ventricle, the heart cut out, and a slit made to open the ventricular cavity. The heart was held just above the auricular ventricular groove by a screw-clamp, and the free end of the piece of fine silk attached to the ventricle tied to a lever, which has as its fulcrum the rod of the stand holding the screw-clamp. This lever marked by means of a fine piece of aluminium upon the smoked surface of a revolving cylinder.

Each contraction of the ventricle was thus indicated upon the cylinder by a rise in the curve, any increase in the tone being similarly indicated.

The ventricle was first washed with normal solution to see its effect, as it was in this that the β lutidine was always dissolved, it being necessary to determine that the effects produced were not due to the solvent. This saline solution caused a slight fall of the lever, but immediately that containing the β lutidine (about 0·1 per cent.) was applied, a small drop being sufficient, there followed a distinct increase in the tonicity of the ventricle, the curve of beats being raised from the abscissa line, as would naturally occur with this method of work-


II
blood
0.2% β lutidine and blood
blood
0.2% β lutidine and blood
Half size
seconds marker
III
1
2
3
IV
a
4
3
2
1
a

ing. The original beats were restored by washing with normal saline solution.

In III are two continuous parts of a tracing taken by this method, and should be read from right to left. From 1 the curve is due to the beats of the ventricle after washing with a very small quantity of normal saline solution. At 2 a very small drop of 0·2 per cent. β lutidine was allowed to fall on the ventricle, and the changes in the beats, as above described, at once appear, the normal beats being again restored by washing with normal saline solution as at 3. In the particular experiment from which this tracing was taken the drum was slightly irregular, hence the apparent quickening of the beats after the second washing with the saline solution.

Measurement.—The ventricle was just exposed, and during the intervals between the observations the opening was covered with damp blotting paper. Even with this rough method it was found that the injection of 2·0 per cent. β lutidine into various lymph spaces or into the cutaneous vein, caused a distinct decrease in the length of the ventricle, and this was always attended with a slight paleness.

From all these cases it cannot but be evident that the introduction of β lutidine into the system, enabling it to reach the heart, causes a distinct increase in the tonicity of its ventricle.

The auricles seem to be affected but very little, and even continue to beat after the ventricle has stopped.

On Voluntary Muscle.—By a method somewhat similar to that of Dr. Gaskell, the effect of the alkaloid was tried upon ordinary striped muscular tissue. A frog was pithed and placed on its belly in a moist chamber. The tendo-achilles laid bare and tied by fine silk to a lever, the knee-joint being fixed. On washing the muscle with 0·75 per cent. saline solution, as was done to the ventricle, a similar fall of the lever occurred; but on allowing a drop of 0·5 per cent. β lutidine (in normal saline) to fall on it, a distinct rise followed.

Tracing IV illustrates the above, *aa*, being the abscissa line; any increase in the tonicity of the muscle will be indicated by an increase in the distance of the lever's marker from this line. The tracing should be read from right to left. At (1) 0·75 per cent. saline solution was added, and after a short interval the drum allowed to move, a fall of the lever became evident. At (2) and (3) a small drop of 0·5 per cent. of the alkaloid was added, and a distinct rise followed the washing again with normal saline solution at (4) resulting in fall.

(2.) *Time of Heart's Beats.*—At times we found the results of an experiment undecided, but by far the greater number of them indicated a slight slowing after the addition of the alkaloid. It appeared distinctly in the experiments made with strong solutions.

(3.) *Nerves of the Heart.*—In all experiments relating to the action of poisons on the heart one naturally turns to trying their effect upon the vagus. Under ordinary conditions this nerve has the effect when slightly stimulated of slowing, when strongly stimulated of stopping the heart's beat.

The apparatus used was the moist chamber before described, with the addition of a pair of platinum electrodes connected with the secondary coil of a Du Bois Raymond's induction apparatus. The key for short-circuiting the secondary current carried a marker to make a tracing on the revolving cylinder, indicating the point of time at which the vagus was stimulated and the duration of the stimulus by a rise in the line traced on the drum. To ensure the constant strength of the stimulus, we always allowed the Daniells' cell to work for about half an hour before using the coil.

The frog was pithed, the heart and vagus laid bare with as little exposure of the surrounding parts as possible, the lever and moist chamber arranged, and a tracing taken of the normal ventricular beats. The vagus was next stimulated with the secondary coil at various distances from the primary, till one was found at which the heart was slowed and another at which it was stopped.

Experiments were then made with different quantities of solutions of β lutidine of various strengths, from 0·2 cub. centim. of 2 per cent. to 0·2 cub. centim. of 5 per cent., which were injected into various parts of the body, the best results being obtained by injecting into the lower part of the abdominal cavity.

In all cases the alkaloid deprived the vagus of its power, when stimulated, to arrest the beat of the heart.

As an example, a frog was prepared and the vagus found to cause slowing of the heart's beat when stimulated with the secondary at 14·5, and stoppage at 14 and 13·5—a long stoppage at the latter. 0·2 cub. centim. of 2 per cent. solution of β lutidine were injected into the abdominal cavity near the right leg; after three minutes the vagus on stimulation had but very slight effect, and after six minutes no action. From time to time the secondary was moved nearer and nearer the primary, but stimulation had little or no effect, and after twenty-five minutes the action was imperceptible even when the secondary coil was right over the primary.

Reflex Action.

Our next series of experiments were made to ascertain what effect this alkaloid would have upon reflex *action*.

The frogs used had their brains destroyed, with as small a loss of blood as possible, by dividing the spinal cord at the junction of the first vertebra and passing a wire into the skull. The animals were then left for some time to recover from the effects of the operation. The stimulus used was a very weak solution of hydric sulphate, and the time elapsing between the placing of the foot in this stimulant and its withdrawal by the animal was measured by a metronome beating 120 to the minute and taken as the indication of the time required for reflex action. The frog was worked upon either resting on its belly with its legs hanging down or suspended by its head. In order that the results might not be thought due to fatigue, owing to the suspension of the animal, in the earlier experiments, another frog similarly pithed, was suspended, and its reflex time measured simultaneously with the one treated with the alkaloid. These frogs suffered no change, therefore after the first experiments their use was discontinued.

After suspension of the animal its reflex action was tested for half an hour at intervals of five minutes, in order to get the length of its normal reflex time. The alkaloid was then injected, sometimes pure, into the stomach, and at other times dilute into the dorsal lymphatic sac; here the effect could be produced by 0·2 cub. centim. of 1 per cent. solution in ten minutes to a quarter of an hour.

The action of the β lutidine was most distinct, the reflex time, after an interval, growing longer and longer at each stimulation, then the leg would be only just removed, and that feebly from the acid; and, finally, after a time varying from a quarter to half hour the foot was incapable of being withdrawn though immersed in the acid for a minute or a minute and a half.

Decidedly, then, β lutidine destroys reflex action in the frog.

The disappearance of the reflex action might be attributed to the following causes:—

(1.) To the poison acting upon the heart, perhaps stopping its beat, and thus cutting off the blood supply to the spinal cord. In all cases, however, the frog was dissected and the heart was invariably found beating.

(2.) It might be due to its action on the nerves, thus preventing the conduction of impulses to the muscles of the legs, necessary to cause them to contract and withdraw the foot from the acid.

(3.) It might be said to be due to an effect like that of urari, preventing the communication of an impulse from the nerve to the muscle, blocking their physiological communication. That it was not due to either of these causes will be seen from the following experiment: A frog was very carefully pithed, the sciatic nerve on each side was exposed and a ligature tied round the whole of the left leg—except the nerve. The nerves on each side were each stimulated with such a feeble current as to cause only a slight twitching of the toes (secondary coil at about 32).

Far more β lutidine than is necessary to produce the described effect upon reflex action was then injected into the dorsal lymphatic sac, at times as much as two or more drops of the pure alkaloid being used. Although the experiment lasted in some cases over two hours, the feeble stimulus caused the same amount of movement in both feet as before.

(4.) The effect of the poison might also be thought due to its action upon the muscles themselves, causing them to lose their irritability and cease to respond to stimuli reaching them from the nerve. The last experiments would go strongly against this, but to be perfectly sure we performed the following: A frog, whose brain had been destroyed, received urari up to a point at which no reflex action occurred, and no contraction of the leg muscles on stimulation of the sciatic nerve. The animal was placed in a moist chamber, and by means of the pendulum myograph tracings were taken of the contractions of the gastrocnemius, caused by direct stimulation. A number of these tracings of the muscular contraction was taken before application of the poison, and then from time to time β lutidine was injected into the dorsal lymphatic sac, commencing with 0·1 cub. centim. of 10 per cent. solution, and finally using as much as two drops of the pure alkaloid; but there was no alteration of a decided character in any of the curves, either upon the latent period, contraction, or relaxation.

It may be mentioned that after acting on frogs retaining their spinal cord with β lutidine, destruction of the spinal cord with a wire, causes little or no movement of the limbs, as is the case under ordinary conditions.

From all these results it cannot but be evident that the disappearance of reflex action is due alone to the effect of the poison on the nerve cells of the spinal cord.

Strychnine and β Lutidine.—Such being the action of β lutidine upon the spinal cord, we were naturally led to ascertain whether it was antagonistic in its action to strychnine. If a frog, whose brain has been destroyed, be treated with a small quantity of strychnine solution, the slightest stimulus is sufficient to throw the animal into strong tetanic convulsions.

Various experiments were tried; as examples we will give the following:—

(1.) A frog whose reflex action was good (the brain having been destroyed in the usual way) had sufficient β lutidine solution given it, to cause this reflex action to disappear. A drop of 0·5 per cent. strychnine was injected into its dorsal lymphatic sac, a quantity which injected into another frog similarly prepared brought on the strongest convulsions. Still no effect was produced, neither did any change appear when another large drop was injected.

(2.) As an example of another method, a frog (whose brain had been destroyed) was thrown into strong convulsions by the use of strychnine, then 0·5 cub. centim. of 10 per cent. β lutidine were injected into the dorsal lymphatic sac, also a drop of the pure alkaloid; in fifteen minutes the strychnine tetanus was passing off and in twenty-five minutes had quite gone. The same effect happened with smaller quantities of the alkaloid.

(3.) Another, better, method was used. Two frogs, whose brains had been destroyed as before, were taken, and into the dorsal lymphatic sac of each a small drop of 5 per cent. strychnine was injected. A minute afterwards only 0·1 cub. centim. of 10 per cent. β lutidine was injected into one frog (A), the other (B) remaining as before.

After fifteen minutes (A) gave no signs of strychnine tetanus, while (B) gave distinct signs. After twenty minutes (A) gave only very faint reflex action; but (B), on being touched, went into strong tetanus. After thirty minutes (A) gave no signs of reflex action, while (B) went into strong tetanic convulsions on simply touching the table.

These results lasted over an hour; then into (B) 0·1 cub. centim. of 10 per cent. β lutidine was injected, and in ten minutes the effect of the strychnine began to pass off, and in thirty minutes was quite gone, the frog not even giving signs of reflex action.

Again, in some experiments strychnine was injected into one frog (prepared as before), and strychnine with β lutidine into another. After twenty-four hours the first went into strong tetanic convulsions on touching, but never the latter; that of the former disappearing after the injection of the alkaloid.

From our experiments we hope we have made it clear that β lutidine causes a distinct increase in the tonicity of both cardiac and voluntary muscular tissue, also a slowing in the rate of the heart's beat, and that it arrests the inhibitory power of the vagus. That by its action upon the nerve cells of the spinal cord, it, in the first place, lengthens the time of reflex action, and then arrests that function; finally, that it is successfully antagonistic to strychnine in its action upon the spinal cord.

In conclusion, we feel much pleasure in acknowledging our grateful thanks to Dr. Michael Foster, both for his kind help and happy suggestions, which have been of great assistance to us in our investigations.

II. "Discussion of the Results of some Experiments with Whirled Anemometers." By Professor G. G. STOKES, Sec. R.S. Received April 26, 1881.

In the course of the year 1872, Mr. R. H. Scott, F.R.S., suggested to the Meteorological Committee the desirability of carrying out a series of experiments on anemometers of different patterns. This suggestion was approved by the Committee, and in the course of same year a grant was obtained by Mr. Scott from the Government Grant administered by the Royal Society, for the purpose of defraying the expenses of the investigation. The experiments were not, however, carried out by Mr. Scott himself, but were entrusted to Mr. Samuel Jeffery, then Superintendent of the Kew Observatory, and Mr. G. M. Whipple, then First Assistant, the present Superintendent.

The results have never hitherto been published, and I was not aware of their nature till on making a suggestion that an anemometer of the Kew standard pattern should be whirled in the open air, with a view of trying that mode of determining its proper factor, Mr. Scott informed me of what had already been done, and wrote to Mr. Whipple, requesting him to place in my hands the results of the most complete of the experiments, namely, those carried on at the Crystal Palace, which I accordingly obtained from him. The progress of the enquiry may be gathered from the following extract from Mr. Scott's report in returning the unexpended balance of the grant.

"The comparisons of the instruments tested were first instituted in the garden of the Kew Observatory. This locality was found to afford an insufficient exposure.

"A piece of ground was then rented and enclosed within the Old Deer Park. The experiments here showed that there was a considerable difference in the indications of anemometers of different sizes, but it was not possible to obtain a sufficient range of velocities to furnish a satisfactory comparison of the instruments. Experiments were finally made with a rotating apparatus, a steam merry-go-round, at the Crystal Palace, which led to some results similar to those obtained by exposure in the Deer Park.

"The subject has, however, been taken up so much more thoroughly by Drs. Dohrandt and Thiesen (*vide* "Repertorium für Meteorologie," vols. iv and v) and by Dr. Robinson in Dublin, that it seems unlikely that the balance would ever be expended by me. I, therefore, return it with many thanks to the Government Grant Committee.

"The results obtained by me were hardly of sufficient value to be communicated to the Society."

On examining the records, it seemed to me that they were well deserving of publication, more especially as no other experiments of the same kind have, so far as I know, been executed on an anemometer of the Kew standard pattern. In 1860 Mr. Glaisher made experiments with an anemometer whirled round in the open air at the end of a long horizontal pole,* but the anemometer was of the pattern employed at the Royal Observatory, with hemispheres of 3·75 inches diameter and arms of 6·725 inches, measured from the axis to the centre of a cup, and so was considerably smaller than the Kew pattern. The experiments of Dr. Dohrandt and Dr. Robinson were made in a building, which has the advantage of sheltering the anemometer from wind, which is always more or less fitful, but the disadvantage of creating an eddying vorticose movement in the whole mass of air operated on; whereas in the ordinary employment of the anemometer the eddies it forms are carried away by the wind, and the same is the case to a very great extent when an anemometer is whirled in the open air in a gentle breeze. Thus, though Dr. Robinson employed among others an anemometer of the Kew pattern, his experiments and those of Mr. Jeffery are not duplicates of each other, even independently of the fact that the axis of the anemometer was vertical in Mr. Jeffery's and horizontal in Dr. Robinson's experiments; so that the greater completeness of the latter does not cause them to supersede the former.

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* "Greenwich Magnetical and Meteorological Observations," 1862, Introduction, p. li.

In Mr. Jeffery's experiments the anemometers operated on were mounted a little beyond and above the outer edge of one of the steam merry-go-rounds in the grounds of the Crystal Palace, so as to be as far as practicable out of the way of any vortex which it might create. The distance of the axis of the anemometer from the axis of the "merry" being known, and the number of revolutions (n) of the latter during an experiment counted, the total space traversed by the anemometer was known. The number (N) of *apparent* revolutions of the anemometer, that is, the number of revolutions *relatively to the merry*, was recorded on a dial attached to the anemometer, which was read at the beginning and end of each experiment. As the machine would only go round one way, the cups had to be taken off and replaced in a reverse position, in order to reverse the direction of revolution of the anemometer. The *true* number of revolutions of the anemometer was, of course, $N+n$, or $N-n$, according as the rotations of the anemometer and the machine were in the same or opposite directions.

The horizontal motion of the air over the whirling machine during any experiment was determined from observations of a dial anemometer with 3-inch cups on 8-inch arms, which was fixed on a wooden stand in the same horizontal plane as that in which the cups of the experimental instrument revolved, at a distance estimated at about 30 feet from the outside of the whirling frame. The motion of the centres of the cups was deduced from the readings of the dial of the fixed anemometer at the beginning and end of each experiment, the motion of the air being assumed as usual to be three times that of the cups.

The experiments were naturally made on fairly calm days, still the effect of the wind, though small, is not insensible. In default of further information, we must take its velocity as equal to the mean velocity during the experiment.

Let V be the velocity of the anemometer (*i.e.*, of its axis), W that of the wind, θ the angle between the direction of motion of the anemometer and that of the wind. Then the velocity of the anemometer relatively to the wind will be—

$$\sqrt{V^2-2VW\cos\theta+W^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

The mean effect of the wind in a revolution of the merry will be different according as we suppose the moment of inertia of the anemometer very small or very great.

If we suppose it very small, the anemometer may be supposed to be moving at any moment at the rate due to the relative velocity at that moment, and therefore the mean velocity of rotation of the cups in one revolution of the merry will be that corresponding to the mean relative velocity of the anemometer and the air. If, as is practically the case, W be small as compared with V, we may expand (*a*) in a rapidly converging series according to ascending powers of W. All the odd powers will disappear in taking the mean, and if we neglect the fourth and higher powers we shall have for the mean

$$V+\frac{W^2}{4V},$$

so that $W^2 \div 4V$ is the small correction to be added to the measured velocity of the anemometer in order to correct for the wind.

On the other hand, if the moment of inertia of the anemometer be taken as very great, the rate of rotation of the cups during a revolution of the merry will be sensibly constant. If V' be the velocity of the anemometer relatively to the air, v the velocity of the centre of one of the cups, and if we suppose the rotation of the anemometer resisted by a force of which the moment is F, then, according to Dr. Robinson's researches, we have approximately

$$F=AV'^2-2BvV'-Cv^2.$$

In the present case friction is not taken into account, and instead of F we must take the moment of the effective moving force. Furthermore, it appears from the experiments of Dr. Robinson, in Dublin, that the observations were almost as well satisfied by taking the first two terms only of the above expression for F as by taking all three, and this simplification may be employed with abundantly sufficient accuracy in making the small correction for the wind. We have, therefore—

$$F=AV'^2-2BvV',$$

where V' is given by (*a*). In order that the anemometer may be neither accelerated nor retarded from one revolution of the merry to another, the mean effective force must be *nil;* and taking the means of both sides of the above equation, observing that, in consequence of the supposed largeness of the moment of inertia, v is sensibly constant during one revolution of the merry, we have on employing the approximate value of the mean of V′ or (*a*) already used—

$$0=A(V^2+W^2)-2Bv\left(V+\frac{W^2}{4V}\right).$$

But if U be the constant velocity of air relatively to the anemometer which would make the cups turn round at the same rate, we have similarly—

$$0=AU^2-2BvU.$$

Eliminating Bv/A, between these two equations we get—

$$U\left(V+\frac{W^2}{4V}\right)=V^2+W^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (c),$$

and as the fourth and higher powers of W have been neglected all along, we get from the last—

$$U=V+\frac{3W^2}{4V} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d),$$

so that, on this supposition, the mean correction for the wind is $3W^2/4V$, or three times the correction of the former supposition.

The mean value of the radical (*a*) is given by an elliptic function; but even in an extreme case among the experiments, when the ratio of the velocity of the wind to that of the anemometer is as great as 3 to 5, the error of the approximate expression $V+W^2/4V$ amounts only to about 0·01 mile an hour, which may be quite disregarded. The error in employing (*d*) for the determination of U instead of (*c*) is of about similar amount.

Three anemometers were tried, namely, one of the old Kew standard pattern, one by Adie, and Kraft's portable anemometer. Their dimensions will be found at the heads of the respective tables below. With each anemometer the experiments were made in three groups, with high, moderate, and low velocities respectively, averaging about 28 miles an hour for the high, 14 for the moderate, and 7 for the low. Each group again was divided into two subordinate groups, according as the cups were direct, in which case the directions of rotation of the merry and of the anemometer were opposite, or reversed, in which case the directions of the two rotations were the same.

The data furnished by each experiment were: the time occupied by the experiment, the number of revolutions of the merry, the number of *apparent* revolutions of the anemometer, given by the difference of readings of the dial at the beginning and end of the experiment, and the space S passed over by the wind, deduced from the difference of readings of the fixed anemometer at the beginning and end of the experiment.

The object of the experiment was, of course, to compare the mean velocity of the centres of the cups with the mean velocity of the air relatively to the anemometer. It would have saved some numerical calculation to have compared merely the spaces passed through during the experiment; but it seemed better to exhibit the velocities in miles per hour, so as to make the experiments more readily comparable with one another, and with those of other experimentalists. In the reductions I employed 4-figure logarithms, so that the last decimal in V in the tables cannot quite be trusted, but it is retained to match the correction for W, which it seemed desirable to exhibit to 0·01 mile.

On reducing the experiments with the low velocities, I found the results extremely irregular. I was subsequently informed by Mr. Whipple, that the machine could not be regulated at these low velocities, for which it was never intended, and that it sometimes went round fast, sometimes very slowly. He considered that the experiments in this group were of little, if any, value, and that they ought to be rejected. They were besides barely half as numerous as those of the moderate group. I have accordingly thought it best to omit them altogether.

In the following tables the first column gives the group, H standing for high velocities, M for moderate; the subordinate group, — standing for rotation of the anemometer opposite to that of the machine, + for rotations in the same direction, and lastly the reference number of the experiment in each subordinate group. T gives the duration of the experiment in minutes; *n* the number of revolutions of the machine; N the number of *apparent* revolutions of the anemometer; S the space passed over by the natural wind, in miles. These form the data. From them are calculated: V, the velocity of the anemometer, in miles per hour; W, the velocity of the wind; $W^2/2V$ the mean of the two corrections to be added to V on account of the wind, according as we adopt one or other of the extreme hypotheses as to the moment of inertia of the anemometer, namely, that it is very small or very large. The actual correction will be half the number in this column on the first supposition, and once and a-half on the second. V_1, V_2 denote the velocity of the anemometer, or, in other words, of the artificial wind, corrected for the natural wind on these two suppositions respectively, so that the last two columns give 100 times the ratio of the registered velocity to the true velocity, or the registered as a percentage of the true, the registered velocity meaning that deduced from the velocity of the cups on employing the usual factor 3.

The dials of the first two anemometers read only to 10 revolutions, which is the reason why all the numbers N end with a 0.

The Old Kew Standard.

Diameter of Arms between Centres of Cups 48 inches; Diameter of Cups 9 inches. Fixed to Machine at 22·3 feet from the Axis of Revolution.

Group and No.	T.	*n*.	N.	S.	V.	W.	$\frac{W^2}{2V}$.	$\frac{300v}{V_1}$.	$\frac{300v}{V_2}$.
H−									
1	15	303	1690	0·7	31·17	2·80	0·13	126·9	126·3
2	18	301	1690	0·5	26·63	1·67	0·05	124·1	123·8
3	16	301	1580	0·6	29·96	2·25	0·08	114·2	113·9
4	17	300	1710	1·1	28·11	3·88	0·27	125·9	124·7
5	17	300	1720	1·1	28·11	3·88	0·27	126·8	125·5
6	22	400	2210	1·3	28·96	3·27	0·18	121·4	120·6
7	23	400	2220	1·1	27·70	2·87	0·15	122·1	121·5
8	19	300	1670	0·7	25·14	2·21	0·10	122·6	122·2
9	19	300	1640	0·8	25·14	2·53	0·13	119·8	119·3
10	17	301	1670	0·8	28·20	2·71	0·13	122·1	121·5
11	19	300	1670	0·8	25·14	2·53	0·13	122·6	122·0
Mean...	..	..	..	..	26·75	2·78	0·15	122·6	121·9
H+									
1	17	302	980	0·0	28·29	0·00	0·00	114·2	114·2
2	17	300	960	1·0	28·11	3·53	0·22	112·6	111·7
3	15½	300	1000	1·4	30·82	5·42	0·48	115·7	113·9
4	22½	300	1080	1·8	21·23	4·80	0·54	122·3	119·2
5	19	300	1020	0·7	25·14	2·21	0·10	118·1	117·7
6	16	300	1030	0·9	29·86	3·37	0·19	119·0	118·2
7	18	300	1050	0·8	26·54	2·67	0·13	120·8	120·2
8	18	301	1060	0·6	26·63	2·20	0·09	121·4	121·0
9	18	300	1000	0·7	26·54	2·43	0·11	121·7	121·3
Mean...	..	..	..	..	27·02	2·96	0·21	118·4	117·5
M−									
1	30	300	1650	0·8	15·92	1·60	0·08	120·8	120·2
2	31	300	1670	1·6	15·41	3·10	0·31	121·7	119·3
3	34	300	1570	1·9	14·05	3·01	0·22	112·6	110·1
4	36	300	1540	1·7	13·26	2·83	0·30	110·0	107·6
5	36	300	1540	1·3	13·26	2·17	0·18	110·5	109·0
Mean...	..	..	..	..	14·38	2·54	0·24	115·1	113·2
M+									
1	28	301	880	0·0	17·63	0·00	0·00	102·6	102·5
2	38	300	940	2·0	12·57	3·15	0·38	109·5	106·4
3	38	300	890	1·6	12·57	2·52	0·25	105·7	103·7
4	36	300	990	0·8	13·26	1·33	0·07	115·4	114·9
5	35	300	990	1·0	13·66	1·71	0·07	115·3	114·8
Mean...	..	..	..	..	13·94	1·74	0·15	109·7	108·5

Adie's Anemometer.

Diameter of Arms between Centres of Cups 13·4 inches; Diameter of Cups 2·5 inches. Fixed to Machine at 20·7 feet from the Axis of Revolution.

Group and No.	T.	*n*.	N.	S.	V.	W.	$\frac{W^2}{2V}$.	$\frac{300v}{V_1}$.	$\frac{300v}{V_2}$.
H–									
1	17	300	3860	1·0	26·16	3·53	0·24	95·2	94·4
2	15½	300	3650	1·4	28·61	5·45	0·52	89·5	87·9
3	22½	300	3940	1·8	19·70	4·80	0·58	96·7	94·0
4	19	300	3760	0·7	23·33	2·21	0·10	93·1	92·7
5	16	300	3780	0·9	27·71	3·37	0·20	93·5	92·8
6	18	300	3890	0·9	24·63	2·67	0·14	96·4	96·0
7	18	301	3980	0·7	24·72	2·33	0·11	98·6	98·2
8	18	300	3940	0·8	24·63	2·67	0·14	97·8	97·3
Mean...	..	..	..	..	24·94	3·38	0·25	95·1	94·2
H+									
1	17	300	3240	1·1	26·08	3·89	0·29	94·9	93·9
2	17	300	3330	1·1	26·08	3·89	0·29	97·3	96·3
3	19	300	3760	0·7	23·33	2·21	0·10	109·2	108·7
4	16	300	3780	0·9	27·71	3·37	0·20	109·6	108·8
5	19	300	3060	0·8	23·33	2·53	0·14	90·3	89·8
6	17	301	3120	0·8	26·17	2·82	0·15	91·6	91·1
7	19	300	3160	0·8	23·33	2·53	0·14	93·0	92·5
Mean...	..	..	..	..	25·15	3·03	0·19	98·0	97·3
M–									
1	38	300	3620	2·0	11·67	3·16	0·43	87·7	84·9
2	38	300	3500	1·6	11·67	2·53	0·43	84·7	81·7
3	36	300	3910	0·8	12·26	1·33	0·07	97·5	96·9
4	35	300	3430	1·0	12·67	1·71	0·07	84·1	83·7
Mean...	..	..	..	..	12·07	2·18	0·25	88·5	86·8
M+									
1	31	300	3250	1·6	14·30	3·10	0·34	94·3	93·7
2	34	300	2920	1·7	13·04	3·00	0·35	85·7	85·5
3	34	300	2940	1·9	13·04	3·06	0·36	86·1	83·1
4	36	300	2760	1·7	12·31	2·83	0·33	81·4	79·2
5	36	300	2780	1·3	12·31	2·17	0·19	65·5	64·7
Mean...	..	..	..	..	13·00	2·83	0·31	82·6	81·0

Kraft's Portable Anemometer.

Diameter of Arms between Centres of Cups 8·3 inches; Diameter of Cups 3·3 inches. Fixed to Machine at 19·10 feet from the Axis of Revolution.

Group and No.	T.	*n*.	N.	S.	V.	W.	$\frac{W^2}{2V}$.	$\frac{300v}{V_1}$.	$\frac{300v}{V_2}$.
H−									
1	19	300	5594	0·6	21·53	1·89	0·08	95·6	95·4
2	17½	303	5681	0·7	23·60	2·69	0·15	96·2	95·4
3	15	303	5990	0·7	27·22	2·80	0·14	102·8	102·4
4	18	301	6086	0·5	22·79	1·67	0·06	104·2	104·0
5	16	301	6116	0·6	25·65	2·25	0·10	104·7	104·3
6	17	300	6143	1·1	24·06	3·88	0·31	105·2	103·6
7	17	300	6240	1·1	24·06	3·88	0·31	106·9	105·5
8	22	400	7896	1·3	24·79	3·27	0·21	101·4	100·4
9	23	400	7900	1·1	23·71	2·78	0·16	101·5	100·9
10	19	300	5966	0·7	21·53	2·21	0·11	102·3	101·7
11	19	300	5751	0·8	21·53	2·53	0·15	98·3	97·7
12	17	301	5842	0·8	24·14	2·82	0·16	99·6	99·0
13	19	300	5892	0·8	21·53	2·53	0·15	100·9	100·1
Mean...	..	..	..	..	23·55	2·71	0·16	101·5	100·8
H+									
1	17	300	5372	1·0	24·06	3·53	0·26	102·2	101·0
2	15½	300	5265	1·4	26·39	5·42	0·56	99·7	97·5
3	22½	300	5460	1·8	18·18	4·80	0·63	102·5	98·9
4	19	300	5093	0·7	21·53	2·21	0·11	97·4	96·8
5	16	300	5282	0·9	25·57	3·37	0·22	100·6	99·8
6	18	300	5274	0·8	22·72	2·67	0·16	100·6	99·8
7	18	301	5300	0·6	22·79	2·05	0·09	100·9	100·5
8	18	300	5363	0·8	22·72	2·67	0·16	102·2	101·4
Mean...	..	..	..	..	22·99	3·33	0·27	100·8	99·4
M−									
1	30	300	5488	0·8	13·63	1·60	0·09	93·3	92·5
2	31	300	5880	1·6	13·19	3·10	0·36	99·2	96·6
3	34	300	5168	1·6	12·03	2·53	0·27	86·8	84·8
4	34	300	5320	1·9	12·03	3·35	0·47	88·8	85·4
5	36	300	5030	1·7	11·36	2·83	0·35	84·0	81·4
6	36	300	4910	1·3	11·36	2·17	0·21	82·3	80·9
Mean...	..	..	..	..	12·27	2·60	0·29	89·1	86·9
M+									
1	38	300	4508	2·0	10·76	3·16	0·46	84·8	81·4
2	38	300	4250	1·6	10·76	2·53	0·30	80·9	78·7
3	36	300	5006	0·8	11·36	1·33	0·08	95·3	94·7
4	35	300	4743	1·0	11·69	1·71	0·12	90·3	89·3
Mean...	..	..	..	..	11·14	2·18	0·24	87·8	86·0

The mean results for the high and moderate velocities, contained i the preceding tables, are collected in the following table, in which are also inserted the mean errors.

Anemometer.	Directions of Rotation.	High Velocities.				Moderate Velocities.			
		Mom. inert. small.		Mom. inert. large.		Mom. inert. small.		Mom. inert. large.	
		p. c.	m. e.	p. c.	m. e.	p. c.	m. e.	p. c.	m. e.
Kew.	Opposite....	122·6	2·4	121·9	2·3	115·1	4·9	113·2	5·2
	Alike........	118·4	2·9	117·5	2·8	109·7	4·5	108·5	5·1
	Mean	120·5	..	119·7	..	112·4	..	110·8	
Adie.	Opposite.....	95·1	2·3	94·2	2·3	88·5	4·5	86·8	5·0
	Alike........	98·0	6·5	97·3	6·5	82·6	7·3	81·0	7·3
	Mean	96·5	..	95·7	..	85·5	..	83·9	
Kraft.	Opposite.....	101·5	2·6	100·8	2·5	89·1	4·8	86·9	5·1
	Alike........	100·8	1·2	99·4	1·3	87·8	5·0	86·0	6·0
	Mean	101·1	..	100·1	..	88·4	..	86·4	

The mean errors exhibited in the above table show no great difference according as we suppose the moment of inertia of the anemometer small or large in correcting for the wind. There appears to be a slight indication, beyond what may be merely casual, that the errors are a little greater on the latter supposition than on the former, which is what we should rather expect; for an anemometer would get pretty well under way in a fraction of a revolution of the whirling instrument. However, the difference is so small that it will suffice to take the mean of the two as the mean error belonging to the particular anemometer, class of velocity, and character of rotation under consideration. From the mean errors we may calculate nearly enough, by the usual formulæ, the probable errors of the various mean percentages for rotations opposite and alike. The probable errors of these mean percentages come out as follows:—

Kew, 1·0 for high velocities; 2·7 for moderate velocities.
Adie, 1·5 ,, ,, 2·0 ,, ,,
Kraft, 0·9 ,, ,, 1·8 ,, ,,

These probable errors are so small that it appears that for the high and even for the moderate velocities the experiments are extremely trustworthy, except in so far as they may be affected by *systematic* sources of error.

If we compare the registered percentages of the true velocity of the air relatively to the anemometer according as the rotations are in opposite directions or in the same direction, we see that in five out of the six cases they are slightly greater when the rotations are opposite. The sole exception is in the group "Adie, high velocities," which is made up of the groups "Adie H−" and "Adie H+." On referring to the principal table for the Adie, we see that Experiments 3 and 4 in group H+ give percentages usually high, depending on the high values of N. These raise the mean for the group, and make the mean error far greater than those of the other five groups for high velocities. There appears little doubt, therefore, that the excess of percentages obtained for rotations opposite is real, and not merely casual. It is, however, so small as to give us much confidence in the correctness of the mean result, unless there were causes to vitiate it which apply to both directions of rotation alike.

It may be noticed that the difference is greatest for the Kew, in which the ratio of r to R is greatest, r denoting the radius of the arm of the anemometer, and R the distance of its axis from the axis of revolution of the machine, and appears to be least (when allowance is made for the two anomalous experiments in the group "Adie H+") for the Kraft, for which r/R is least. In the Kraft, indeed, the differences are roughly equal to the probable errors of the means. In these whirling experiments r/R is always taken small, and we might expect the correction to be made on account of the finiteness of R to be expressible in a rapidly converging series according to powers of r/R, say—

$$\mathrm{A}'\frac{r}{\mathrm{R}}+\mathrm{B}'\left(\frac{r}{\mathrm{R}}\right)^2+\mathrm{C}'\left(\frac{r}{\mathrm{R}}\right)^3+\ldots$$

We may, in imagination, pass from the case of rotations opposite to that of rotations alike, by supposing R taken larger and larger in successive experiments, altering the angular velocity of revolution so as to preserve the same linear velocity for the anemometer, and supposing the increase continued until R changes sign in passing through infinity, and is ultimately reduced in magnitude to what it was at first. The ideal case of $\mathrm{R}=\infty$ is what we aim at, in order to represent the motion of a fixed anemometer acted on by perfectly uniform wind by that of an anemometer uniformly impelled in a rectilinear direction in perfectly still air. We may judge of the magnitude of the leading term in the above correction, provided it be of an odd order, by that of the difference of the results for the two directions of rotation. Unless, therefore, we had reason to believe that A′ were 0, or at least very small compared with B′, we should infer that the whole correction for the finiteness of R is very small, and that it is practically eliminated by taking the mean of the results for rotations opposite and rotations alike.

We may accept, therefore, the mean results as not only pretty well freed from casual irregularites which would disappear in the mean of an infinite number of experiments, but also, most probably, from the imperfection of the representation of a rectilinear motion of the anemometer by motion in a circle of the magnitude actually employed in the experiments.

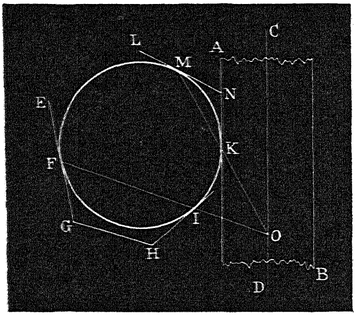
Before discussing further the conclusions to be drawn from the results obtained, it will be well to consider the possible influence of systematic sources of error.

1. *Friction.*—No measure was taken of the amount of friction. nor were any special appliances used to reduce it; the anemometers were mounted in the merry just as they are used in actual registration. Friction arising from the weight is guarded against as far as may be in the ordinary mounting, and what remains of it would act alike in the ordinary use of the instrument and in the experiments, and as far as this goes, therefore, the experiments would faithfully represent the instrument as it is in actual use. But the bearings of an anemometer have also to sustain the lateral pressure of the wind, which in a high wind is very considerable; and the construction of the bearing has to be attended to in order that this may not produce too much friction. So far the whirled instrument is in the same condition as the fixed. But besides the friction arising from the pressure of the artificial wind, a pressure which acts in a direction tangential to the circular path of the whirled anemometer, there is the pressure arising from the centrifugal force. The highest velocity in the experiments was about 30 miles an hour, and at this rate the centrifugal force would be about three times the weight of the anemometer. This pressure would considerably exceed the former, at right angles to which it acts, and the two would compound into one equal to the square root of the sum of their squares. The resulting friction would exceed a good deal that arising from the pressure of the wind in a fixed anemometer with the same velocity of wind (natural or artificial), and would sensibly reduce the velocity registered, and accordingly raise the coefficient which Dr. Robinson denotes by m, the ratio, namely, of the velocity of the wind to the velocity of the centres of the cups. It may be noticed that the percentages collected in the table on p. 179, are very distinctly lower for the moderate velocities than for the high velocities. Such an effect would be produced by friction; but how far the result would be modified if the extra friction due to the centrifugal force were got rid of, and the whirled anemometer thus assimilated to a fixed anemometer, I have not the means of judging, nor again how far the percentages would be still further raised if friction were got rid of altogether.

Perhaps the best way of diminishing friction in the support of an anemometer is that devised and employed by Dr. Robinson, in which the anemometer is supported near the top on a set of spheres of gun-metal contained in a box with a horizontal bottom and vertical side which supports and confines them. For vertical support, this seems to leave nothing to be desired, but when a strong lateral pressure has to be supported as well as the weight of the instrument, it seems to me that a slight modification of the mode of support of the balls might be adopted with advantage. When a ball presses on the bottom and vertical side of its box, and is at the same time pressed down by the horizontal disk attached to the shaft of the anemometer which rests on the balls, it revolves so that the instantaneous axis is the line joining the points of contact with the fixed box. But if the lateral force of the wind presses the shaft against the ball, the ball cannot simply roll as the anemometer turns round, but there is a slight amount of rubbing.

This, however, may be obviated by giving the surfaces where the ball is in contact other than a vertical or horizontal direction.

Let AB be a portion of the cylindrical shaft of an anemometer; CD, the axis of the shaft; EFGHI, a section of the fixed box or cup containing the balls; LMN, a section of a conical surface fixed to the shaft, by which the anemometer rests on its balls; FIKM, a section of one of the balls; F, I, the points of contact of the ball with the box; M, the point of contact with the supporting cone; K, the point of contact or all but contact of the ball with the shaft. The ball is supposed to be of such size that when the anemometer simply rests on the balls by its own weight, being turned perhaps by a gentle wind, there are contacts at the points M, F, I, while at K the ball and shaft are separated by a space which may be deemed infinitesimal. Lateral pressure from a stronger wind will now bring the shaft into contact with the ball at the point K also, so that the box on the one hand and the shaft with its appendage on the other, will bear on the ball at four points. The surface of the box as well as that on the cone LN being supposed to be one of revolution round CD, those four points will be



situated in a plane through CD, which will pass of course through the centre of the ball.

If the ball rolls without rubbing at any one of the four points F, I, K, M as the anemometer turns round, its instantaneous axis must be the line joining the points of contact, F, I, with the fixed box. But as at M and K likewise there is nothing but rolling, the instantaneous motion of the ball may be thought of as one in which it moves as if it were rigidly connected with the shaft and its appendage, combined with a rotation over LNAB supposed fixed. For the two latter motions the instantaneous axes are CD, MK, respectively. Let MK produced cut CD in O. Then since the instantaneous motion is compounded of rotations round two axes passing through O, the instantaneous axis must pass through O. But this axis is FI. Therefore, FI must pass through O. Hence the two lines FI, MK, must intersect the axis of the shaft in the same point, which is the condition to be satisfied in order that the ball may roll without rubbing, even though impelled laterally by a force sufficient to cause the side of the shaft to bear on it. The size of the balls and the inclinations of the surfaces admit of considerable latitude subject to the above condition. The arrangement might suitably be chosen something like that in the figure. It seems to me that a ring of balls constructed on the above principle would form a very effective upper support for an anemometer whirled with its axis vertical. Possibly the balls might get crowded together on the outer side by the effect of centrifugal force. This objection, should it be practically found to be an objection, would not of course apply to the proposed system of mounting in the case of a fixed anemometer. Below, the shaft would only require to be protected from lateral motion, which could be done either by friction wheels or by a ring of balls constructed in the usual manner, as there would be only three points of contact.

2. *Influence on the Anemometer of its own Wake.*—By this I do not mean the influence which one cup experiences from the wake of its predecessor, for this occurs in the whirling in almost exactly the same way as in the normal use of the instrument, but the motion of the air which remains at any point of the course of the anemometer in consequence of the disturbance of the air by the anemometer when it was in that neighbourhood in the next preceding and the still earlier revolutions of the whirling instrument.

It seems to me that in the open air where the air impelled by the cups is free to move into the expanse of the atmosphere, instead of being confined by the walls of a building, this must be but small, more especially as the wake would tend to be carried away by what little wind there might be at the time. On making some enquiries from Mr. Whipple as to a possible verticose movement created in the air through which the anemometer passed, he wrote as follows:—" I feel confident that under the circumstances the tangential motion of the air at the level of the cups was so small as not to need consideration in the discussion of the results. As in one or two points of its revolution the anemometer passed close by some small trees in full leaf, we should have observed any eddies or artificial wind had it existed, but I am sure we did not."

3. *Influence of the Variation of the Wind; first, as regards Variations which are not Rapid.*—During the 20 or 30 minutes that an experiment lasted, there would of course be numerous fluctuations in the velocity of the wind, the mean result of which is alone recorded. The period of the changes (by which expression it is not intended to assert that they were in any sense regularly periodic), might be a good deal greater than that of the merry, or might be comparatively short. In the high velocities, at any rate, in which one revolution took only three or four seconds, the supposition that the period of the changes was large compared with one revolution is probably a good deal nearer the truth than the supposition that it is small.

On the former supposition, the correction for the wind during two or three revolutions of the merry would be given by the formulæ already employed, taking for W its value at the time. Consequently, the total correction will be given by the formulæ already used, if we substitute the mean of W^2 for the square of mean W. The former is necessarily greater than the latter; but how much, we cannot tell without knowing the actual variations. We should probably make an outside estimate of the effect of the variations, if we supposed the velocity of the wind twice the mean velocity during half the duration of the experiment, and nothing at all during the remainder. On this supposition, the mean of W^2 would be twice the square of mean W, and the correction for the wind would be doubled. At the high velocities of revolution, the whole correction for the wind is so very small, that the uncertainty arising from variation as above explained is of little importance, and even for the moderate velocities it is not serious.

4. *Influence of Rapid Variations of the Wind.*—Variations of which the period is a good deal less than that of the revolutions of the whirling instrument act in a very different manner. The smallness of the corrections for the wind hitherto employed depends on the circumstance that with uniform wind, or even with variable wind, when the period of variation is a good deal greater than that of revolution of the merry, the terms depending on the first power of W, which letter is here used to denote the momentary velocity of the wind, disappear in the mean of a revolution. This is not the case when a particular velocity of wind belongs only to a particular part of the circle described by the anemometer in one revolution. In this case there will in general be an outstanding effect depending on the first power of W,

which will be considerably larger than that depending on W^2. Thus suppose the velocity of whirling to be 30 miles an hour, and the average velocity of the wind 3 miles an hour; the correction for the wind supposed uniform, or if variable, then with not very rapid variations, will be comparable with 1 per cent. of the whole; whereas, with rapid variations, the effect in any one revolution may be comparable with 10 per cent. There is, however, this important difference between the two: that whereas the correction depending on the square leaves a positive residue, however many experiments be made, the correction depending on the first power tends ultimately to disappear, unless there be some cause tending to make the average velocity of the wind different for one azimuth of the whirling instrument from what it is for another. This leads to the consideration of the following conceivable source of error.

5. *Influence of Partial Shelter of the Whirling Instrument.*—On visiting the merry-go-round at the Crystal Palace, I found it mostly surrounded by trees coming pretty near it, but in one direction it was approached by a broad open walk. The consequence is, that the anemometer may have been unequally sheltered in different parts of its circular course, and the circumstances of partial shelter may have varied according to the direction of the wind. This would be liable to leave an uncompensated effect depending on the first power of W. I do not think it probable that any large error was thus introduced, but it seemed necessary to point out that an error of the kind may have existed.

The effect in question would be eliminated in the long run if the whirling instrument were capable of reversion, and the experiments were made alternately with the revolution in one direction, and the reverse. For then, at any particular point of the course at which the anemometer was more exposed to wind than on the average, the wind would tend to increase the velocity of rotation of the anemometer for one direction of revolution of the whirling instrument just as much, ultimately, as to diminish it for the other. Mere reversion of the cups has no tendency to eliminate the error arising from unequal exposure in different parts of the course. And even when the whirling instrument is capable of reversion, it is only very slowly that the error arising from partial shelter is eliminated compared with that of irregularities in the wind; of those irregularities, that is to say, which depend on the first power of W. For these irregularities go through their changes a very great number of times in the course of an experiment lasting perhaps half an hour; whereas, the effect of partial shelter acts the same way all through one experiment. It is very desirable therefore, that in any whirling experiments carried on in the open air, the condition of the whirling instrument as to exposure or shelter should be the same all round.

The trees, though taller than the merry when I visited the place last year, were but young, and must have been a good deal lower at the time that the experiments were made. Mr. Whipple does not think that any serious error is to be apprehended from exposure of the anemometer during one part of its course and shelter during another.

From a discussion of the foregoing experiments, it seems to me that the following conclusions may be drawn :—

1. That, at least for high winds, the method of obtaining the factor for an anemometer, which consists in whirling the instrument in the open air is capable, with proper precautions, of yielding very good results.

2. That the factor varies materially with the pattern of the anemometer. Among those tried, the anemometers with the larger cups registered the most wind, or in other words required the lowest factors to give a correct result.

3. That with the large Kew pattern, which is the one adopted by the Meteorological Office, the register gives about 120 per cent. of the truth, requiring a factor of about 2·5, instead of 3. Even 2·5 is probably a little too high, as friction would be introduced by the centrifugal force, beyond what occurs in the normal use of the instrument.

4. That the factor is probably higher for moderate than for high velocities; but whether this is solely due to friction, the experiments do not allow us to decide.

Qualitatively considered, these results agree well with those of other experimentalists. As the factor depends so much on the pattern of the anemometer, it is not easy to find other results with which to compare the actual numbers obtained, except in the case of the Kew standard. The results obtained by Dr. Robinson, by rotating an anemometer of this pattern without friction purposely applied, are given at pp. 797 and 799 of the "Phil. Trans." for 1878. The mean of a few taken with velocities of about 27 miles an hour in still air gave a factor 2·36, instead of 2·50, as deduced from Mr. Jeffery's experiments. As special antifriction appliances were used by Dr. Robinson, the friction in Mr. Jeffery's experiments was probably a little higher. If such were the case, the factor ought to come out a little higher than in Dr. Robinson's experiments, which is just what it does. As the circumstances of the experiments were widely different with respect to the vorticose motion of the air produced by the action of the anemometer in it, we may I think conclude that no very serious error is to be apprehended on this account.

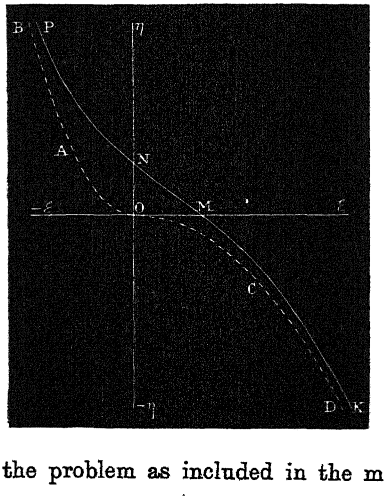
In a later paper (" Phil. Trans." for 1880, p. 1055), Dr. Robinson has determined the factor for an anemometer (among others) of the Kew pattern by a totally different method, and has obtained values considerably larger than those given by the former method. Thus the limiting value of the factor m corresponding to very high velocities, is given at p. 1063 as 2·826, whereas the limiting value obtained by the former method was only 2·286. Dr. Robinson has expressed a preference for the later results. I confess I have always been disposed to place greater reliance on the results of the Dublin experiments, which were carried out by a far more direct method, in which I cannot see any flaw likely to account for so great a difference. It would be interesting to try the second method in a more favourable locality.

I take this opportunity of putting out some considerations respecting the general formula of the anemometer, which may perhaps not be devoid of interest.

The problem of the anemometer may be stated to be as follows:—Let a uniform wind with velocity V act on a cup anemometer of given pattern, causing the cups to revolve with a velocity v, referred to the centre of the cups, the motion of the cups being retarded by a force of friction F; it is required to determine v as a function of V and F, F having any value from 0, corresponding to the ideal case of a frictionless anemometer, to some limit F_1, which is just sufficient to keep the cups from turning. I will refer to my appendix to the former of Dr. Robinson's papers ("Phil. Trans." for 1878, p. 818), for the reasons for concluding that F is equal to V^2 multiplied by a function of V/v. Let

$$V/v=\xi, \qquad F/V^2=\eta,$$

then if we regard ξ and η as rectangular co-ordinates, we have to determine the form of the curve, lying within the positive quadrant $\xi O\eta$, which is defined by those co-ordinates.



We may regard the problem as included in the more general problem of determining v as a function of V and F, where v is positive, but F may be of any magnitude and sign, and therefore, V also.* Negative values of F mean, of course, that the cups instead of being retarded by friction, are acted on by an impelling force making them go faster than in a frictionless anemometer, and values greater than F_1 imply a force sufficient to send them round with the concave sides foremost.

Suppose now F to be so large, positive or negative, as to make v so great that V may be neglected in comparison with it, then we may think of the cups as whirled round in quiescent air in the positive or usual direction when F is negative, in the negative direction when F is greater than F_1. When F is sufficiently large the resistance may be taken to vary as v^2. For equal velocities v it is much greater when the concave side goes foremost, than when the rotation is the other way. For air impinging perpendicularly on a hemispherical cup, Dr. Robinson found that the resistance was as nearly as possibly four times as great when the concave side was directed to the wind as when the convex side was turned in that direction.† When the air is at rest and the cups are whirled round, some little difference may be made by the wake of each cup affecting the one that follows. Still we cannot be very far wrong by supposing the same proportion, 4 to 1, to hold good in this case. When F is large enough and negative, F may be taken to vary as v^2, say to be equal to $-Lv^2$. Similarly, when F is large enough and positive, F may be taken equal to $L'v^2$, where in accordance with the experiment referred to, L′ must be about equal to 4L. Hence we must have nearly—

$$\eta = -L\xi^2, \text{ when } \xi \text{ is positive and very large;}$$
$$\eta = 4L\xi^2 \quad ,, \quad \text{negative} \quad ,, \quad ,,$$

Hence if we draw the semi-parabola OAB corresponding to the equation $\eta = 4L\xi^2$ in the quadrant $\eta O - \xi$, and the semi-parabola OCD with a latus lectum four times as great in the quadrant $\xi O - \eta$, our curve at a great distance from the origin must nearly follow the parabola OAB in the quadrant $\eta O - \xi$, and the parabola OCD in the quadrant $\xi O - \eta$, and between the two it will have some flowing form such as PNMK. There must be a point of inflexion somewhere between P and K, not improbably within the positive quadrant $\xi O \eta$. In the neighbourhood of this point the curve NM would hardly differ from a straight line. Perhaps this may be the reason why Dr. Robinson's experiments in the paper published in the "Phil. Trans." for 1878 were so nearly represented by a straight line.

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* Of course v must be supposed not to be so large as to be comparable with the velocity of sound, since then the resistance to a body impelled through air, or having air impinging on it, no longer varies as the square of the velocity.

† "Transactions of the Royal Irish Academy," vol. xxii, p. 163.

III. "Investigations on the Spectrum of Magnesium." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received April 28, 1881.

[PLATE 1.]

Since our last communication on this subject ("Proc. Roy. Soc.," vol. 30, p. 93) several authors—Ciamician, Cornu and Fievez—have published observations on the spectrum of magnesium, to some of which allusion is made in the sequel, but these observations by no means exhaust the subject. Our own observations, carried on for a considerable time, have extended to new regions and a variety of circumstances, and the summary of them which we now present to the Society will, we hope, help to bring out the connexion between some of the variations in the spectrum of this element and the conditions under which it is observed, and throw additional light on the question of the emissive power for radiations of short wave-length of substances at the relatively low temperature of flame to which we alluded in our paper on the spectrum of water ("Proc. Roy. Soc.," vol. 30, p. 580).

We begin with an account of these observations.

Spectrum of the Flame of Burning Magnesium.

When magnesium wire or ribbon is burnt in air, we see the three lines of the *b* group, the blue line about wave-length 4570, first noticed by us in the spark spectrum ("Proc. Roy. Soc.," vol. 27, p. 350); and photographs show, besides, the well-known triplet in the ultraviolet between the solar lines K and L sharply defined, and the line for which Cornu has found the wave-length 2850 very much expanded and strongly reversed. These lines are all common to the flame, arc, and spark spectra; and the last of them (2850) seems to be by far the strongest line both in the flame and arc, and is one of the strongest in the spark. But, in addition to these lines, the photographs of the flame show a very strong, somewhat diffuse, triplet, generally resembling the other magnesium triplets in the relative position of its components, close to the solar line M; and a group of bands below it extending beyond the triplet near L. These bands have, for the most part, each one sharply defined edge, but fade away on the other side; but the diffuse edges are not all turned towards the same side of the spectrum. The positions of the sharp edges of these bands, and of the strong triplet near M, are shown in the figure, No. 1. It is remarkable that the triplets near P and S are absent from the flame spectrum, and that the strong triplet near M is not represented at all either in the arc or spark. The hydrogen-magnesium series of lines, beginning at a wave-length about 5210, are also seen sometimes, as already described by us ("Proc. Roy. Soc.," vol. 30, p. 96), in the spectrum of the flame; but we have never observed that the appearance of these lines, or of the strong line with which they begin, is connected with the non-appearance of b_4. Indeed, we can almost always see all three lines of the b group in the flame, though as b_4 is the least strong of the three, it is likely to be most easily overpowered by the continuous spectrum of the flame. The new observations recorded below leave, we think, no room for doubt that the series of lines beginning at wave-length 5210 are due to a combination of hydrogen with magnesium, and are not dependent solely on the temperature.

The wave-lengths of the strong triplet near M are about 3720, 3724, 3730, and of the defined edges of the bands about 3750, 3756, 3765, 3772, 3777, 3782, 3790, 3799, 3806, 3810, 3815, 3824, 3841, 3845, 3848, 3855, 3858, 3860, 3865.

Burning magnesium in oxygen instead of atmospheric air does not bring out any additional lines; on the contrary, the continuous spectrum from the magnesia overpowers the line spectrum, and makes it more difficult of observation.

Magnesia heated in the oxyhydrogen jet does not appear to give the lines seen in the flame.

We have left out of the figure and from the enumeration of lines the well-known bands of the oxide.

Spectrum of the Arc.

By examining the arc of a battery of 40 Grove's cells, or that of a Siemens' machine, taken in a crucible of lime, under the dispersion of the spectrum of the fourth order given by a Rutherford grating of 17,296 lines to the inch, we are able to separate the iron and magnesium lines which form the very close pair b_4 of the solar spectrum. Either of the two lines can be rendered the more prominent of the pair at will, by introducing iron or magnesium into the crucible. The less refrangible line of the pair is thus seen to be due to iron, the more refrangible to magnesium. Comparison of the solar line and the spark between magnesium points confirms this conclusion, that the magnesium line is the more refrangible of the two.

In the ultra-violet part of the spectrum photographs show several new lines. First, a pair of lines above U at wave-lengths about 2942, 2938·5.* These lines are a little below a pair of lines given by the spark for which Cornu has found the wave-lengths 2934·9, 2926·7. The latter pair are not seen at all in photographs of the arc, nor the former in those of the spark. The strong line, wave-length about

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[*] [Many of our photographs show besides these two lines a third line wave-length about 2937·5, but we have not been able to determine certainly that it is due to magnesium. If so this group probably belongs to the series of triplets.—June 2.]

2850, is always seen, very frequently reversed. Of the quadruple group in the spark to which Cornu has assigned the wave-lengths 2801·3, 2797·1, 2794·5, and 2789·9, the first and third are strongly developed in the arc, the other two not at all. Next follows a set of five nearly equidistant lines, well-defined and strong, but much less strong than the two previously mentioned, wave-lengths about 2782·2, 2780·7, 2779·5, 2778·2, 2776·9. The middle line is a little stronger than the others. The same lines come out in the spark.

Beyond these follow a series of pairs and triplets; probably they are triplets in every case, but the third, most refrangible, line of the triplets is the weakest, and has not in every case been noticed as yet. These succeed one another at decreasing intervals with diminishing strength, and are alternately sharp and diffuse, the diffuse triplets being the strongest. The positions are shown in fig. 2. The series resembles in general character the sodium and the potassium series described by us in a former communication, and we cannot resist the inference that they must be harmonically related, though they do not follow a simple harmonic law. The most refrangible line in the figure at wave-length 2605 represents a faint diffuse band which is not resolvable into lines; it belongs, no doubt, to the diffuse members of the series, and, to complete the series, there should be another sharp group between it and the line at wave-length 2630. This belonging to the weaker members of the series is too weak to be seen.

The approximate wave-lengths found by us for these lines are as follows:—2767·5, 2764·5, 2736, 2732·5, 2731, 2698, 2695, 2693·5, 2672·5, 2670, 2668·5, 2649, 2646, 2633, 2630, 2605.

It is worthy of remark that the line at wave-length 5710, described by us in a previous communication ("Proc. Roy. Soc.," vol. 30, p. 98), is very nearly the octave of the strong line at 2850. Moreover the measures we have taken of the wave-length of this last line, with a Rutherford grating of 17,296 lines to the inch, indicate a wave-length 2852 nearly, which is still closer to the half of 5710.

In Cornu's map of the solar spectrum a line is ascribed to magnesium with the wave-length 3278. Although a line at this place appears in many of our photographs of the arc, we have not been able to identify it as a line due to magnesium. It does not show any increased strength when magnesium is introduced into the arc. When metallic magnesium is dropped into a crucible of magnesia or lime through which the arc is passing, the electric current seems sometimes to be conducted chiefly or entirely by the vaporised metal, so that the lines of other metals almost or wholly disappear; but the line at wave-length 3278 does not in such cases appear, though the other magnesium lines are very strongly developed. The line at wave-length 2850 is often, under such circumstances, enormously expanded and reversed, those at wave-lengths 2801, 2794, and the alternate diffuse triplets, including those near L and near S, much expanded and reversed, and the group of five lines (2776—2782) sometimes reversed.

When the arc of a Siemens machine is taken in a magnesia crucible, the strong line of the flame spectrum, wave-length 4570, is well seen sharply defined; it comes out strongly and a little expanded on dropping in a fragment of magnesium. When a gentle stream of hydrogen is led in through a hollow pole, this line is frequently reversed as a sharp black line on a continuous background. From comparing the position of this line with those of the titanium lines in its neighbourhood, produced by putting some titanic oxide into the crucible, we have little doubt that it is identical with the solar line 4570·9 of Ångström.

When the arc is taken in a crucible into which the air has access, it may be assumed that the atmosphere about the arc is a mixture of nitrogen and carbonic oxide. When a stream of hydrogen is passed, either through a perforated pole or by a separate opening, into the crucible, the general effect is to shorten the length to which the arc can be drawn out, increase the relative intensity of the continuous spectrum, and diminish the intensity of the metallic lines. Thus, with a very gentle stream of hydrogen in a magnesia crucible, most of the metallic lines, except the strongest and those of magnesium, disappear. Those lines which remain are sometimes reversed; those at wave-length 2850 and the triplet near L being always so. With a stronger stream the lines of magnesium also disappear, the b triplet being the last in that neighbourhood to go, and b_1 and b_2 remaining after b_4 has disappeared.

Chlorine seems to have an opposite effect to hydrogen, generally intensifying the metallic lines, at least those of the less volatile metals, but it does not sensibly affect the spectrum of magnesium. Nitrous oxide produces no marked effect; coal-gas acts much as hydrogen.

Spectrum of the Spark.

In the spark of an induction coil taken between magnesium points in air we get all the lines seen in the arc except two lines at wave-lengths 4350 and 4166, two lines above U, and the series of triplets more refrangible than the quintuple group about wave-length 2780. The blue line wave-length about 4570 is seen in the spark without a jar when the magnesium electrodes are close together, and the rheotome made to work slowly, but requires for its detection a spectroscope in which the loss of light is small.

On the other hand, some additional lines are seen. Of these, the strong line at wave-length 4481 and the weak line at 4586 are well known. Another faint line in the blue at wave-length 4808* has been observed by us in the spark, and two diffuse pairs between H and the triplet near L. Two ultra-violet lines at wave-lengths 2934·9, 2926·7 (Cornu) are near, but not identical with, two lines of the arc above mentioned; and two more lines at wave-lengths 2797·1, 2789·9 (Cornu) make a quadruple group with the very strong pair which are conspicuous in the arc in this region. The spectrum of the spark ends, so far as we have observed, with the quintuple group (2782—2776) already described in the arc. The lines of this spectrum are given in fig. 3.

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* This line we first noticed in a former communication (" Proc. Roy. Soc.,"

When a Leyden jar is used with the coil, some of the lines are reversed. This is notably the case with the triplet near L, the line at wave-length 2850, and those at 2801 and 2794. Cornu ("Compt. Rend.," 1871) noticed the reversal of the less refrangible two lines of the triplet near L under these circumstances. This effect is very much increased by increasing the pressure of the gas in which the spark is taken. For the purpose of observing the influence of increased pressures, we have used a Cailletet pump and glass tubes similar to those employed in the liquefaction of gases by means of such a pump, but with an expansion of the upper part in which were magnesium electrodes attached to platinum wires sealed into the glass. The tube having been filled with gas at the atmospheric pressure, was sealed at its upper end, while the lower end dipped into mercury contained in the iron bottle of the Cailletet pump, and the gas was afterwards compressed by driving more or less mercury into the tube. The gases used were hydrogen, nitrogen, and carbonic oxide; and the image of the spark was thrown on to the slit of the spectroscope by a lens. In hydrogen, when no Leyden jar was used, the brightness of the yellow and of the blue lines of magnesium, except at first that at wave-length 4570, diminished as the pressure increased; while, on the other hand, the *b* group was decidedly stronger at the higher pressure. The pressure was carried up to 20 atmospheres, and then the magnesium lines in the blue and below almost or entirely disappeared, leaving only the *b* group very bright, and the magnesium-hydrogen bands which are described below; even the hydrogen lines F and C were not visible. When a jar was used, the magnesium lines expanded as the pressure was increased; all three lines of the *b* group were expanded and reversed at a pressure of 5 atmospheres; the yellow line, wave-length 5528, was also expanded but not reversed; and the line at 4481 became a broad, very diffuse band, but the line at wave-length 4570 was but very little expanded. The expansion both of the *b* group and of the yellow line seemed to be greater on the less refrangible than on the more refrangible side of each line, so that the black line in those

vol. 27, p. 353), but the wave-length is there given, through an error in taking out the ordinate of the curve of interpolation, as 4797 instead of 4807. Another measure has given the wave-length 4808.

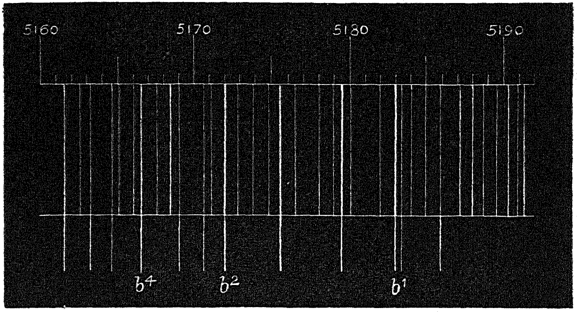
which were reversed was not in the middle. When the jar was used the pressure could not be carried beyond 10 or 12½ atmospheres, as the resistance became then so great that the spark would not pass across the small distance of about 1 millim. between the electrodes. At a pressure of 2½ atmospheres, with a jar, the ultra-violet magnesium triplet near L was very well reversed, and the two pairs of lines on its less refrangible side (shown in fig. 3) were expanded into two diffuse bands.

In nitrogen and in carbonic oxide the general effects of increased pressure on the magnesium lines (not the magnesium-hydrogen bands) seemed to be much the same as in hydrogen. Without a jar the blue and yellow lines were enfeebled, and at the higher pressures disappeared, while the *b* group was very brilliant but not much expanded. With the jar all the lines were expanded, and all three lines of the *b* group strongly reversed. The bands of the oxide (wave-length 4930—5000) were not seen at all in hydrogen or nitrogen; they were seen at first in carbonic oxide, but not after the sparking had been continued for some time.

The disappearance of certain lines at increased pressure is in harmony with the observations of Cazin ("Phil. Mag.," 1877, vol. iv, 154), who noticed that the banded spectrum of nitrogen, and also the lines, grew fainter as the pressure was increased, and finally disappeared. When a Leyden jar is employed there is a very great increase in the amount of matter volatilised by the spark from the electrodes, as is shown by the very rapid blackening of the sides of the tube with the deposited metal, and this increase in the amount of metallic vapour may reasonably be supposed to affect the character of the discharge, and conduce to the widening of the lines and the reversal of some of them. Without a jar the amount of matter carried off the electrode also doubtless increases with the pressure and consequent resistance, and may be the cause of the weakening, as Cazin suggests, of the lines of the gas in which the discharge is passed. It is to be noted, moreover, that the disappearance of the hydrogen lines depends, in some degree, on the nearness of the electrodes. The lines C and F which were, as above stated, sometimes invisible in the spark when the electrodes were near, became visible, under circumstances otherwise similar, when the magnesium points had become worn away by the discharge.

M. Ch. Fievez ("Bull. de l'Académie Royale de Belgique," 1880, p. 91) has investigated the variations in the appearance of the spark spectrum of magnesium under certain different conditions. Using a Rutherford grating of 17,296 lines to the inch, he has noticed certain lines about the *b* group which increase in number with the order of the spectrum observed. He has also noticed dark lines in the solar spectrum corresponding to these lines of magnesium when the two spectra were superposed (fig. 5). We have noticed similar lines in the spectrum of magnesium given by a Rutherford grating, but attribute them to a different cause. The Rutherford gratings have a periodic inequality in the ruling, due to an imperfection of the screw of the ruling machine, in consequence of which the image of every bright line is accompanied by a series of fainter images at nearly equal distances on either side of it, diminishing rapidly in brightness as they recede from the principal line. These *ghosts* are so much fainter than the principal lines that they are not noticed in the case of any but bright lines, and except in the case of very bright lines only two, one on each side, are seen to accompany each principal line.

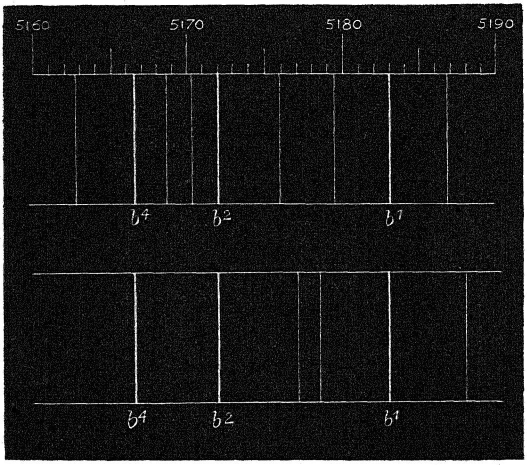
FIG. 5.



Solar spectrum by Rutherford grating 4th order after M. Fievez.

Magnesium.

FIG. 7.



Magnesium *b* group with ghosts produced by Rutherford grating.

Spectrum of 4th order.

Spectrum of 3rd order.

FIG. 6.

The positions of these *ghosts* have been investigated by Mr. Peirce in the "Mathematical Journal" of the Johns Hopkins University, Baltimore, who has found theoretically, and confirmed it by actual observation, that the distance between successive images of the same line is directly proportional to the dispersion and inversely as the order of the spectrum. Our own observations of the positions of the *ghosts* of the *b* group of magnesium lines in spectra of different orders agree closely with Mr. Peirce's theory, and two different Rutherford gratings both give us the same results. The annexed diagram (figs. 6 and 7) gives the relative positions of the first pair of *ghosts* of each of the lines of the *b* group in the spectra of the third and fourth orders, when the angle between collimator and telescope is 45°. If this is compared with M. Ch. Fievez's map, it will be seen that he has probably been deceived by these *ghosts*, both in the solar spectrum and in that of the spark; but as he does not state the angle between his collimator and telescope, no exact comparison can be made. These ghosts are sometimes very embarrassing when many lines are in the field of view, but they may be detected by comparing the spectra of different orders, as the ghosts have different relative positions in the spectra of different orders. In the spectrum of the third order the first ghost of b_2 on the more refrangible side falls on b_4, and that of b_4 on its less refrangible side falls on b_2.

The Magnesium-hydrogen Spectrum.

In the "Proc. Roy. Soc.," vol. 27, p. 494, and vol. 30, p. 93, we have recorded a series of experiments which led us to attribute to magnesium together with hydrogen a peculiar spectrum. This spectrum we have on no occasion been able to detect in the absence of hydrogen. Observations on the spark discharge in nitrogen, in carbonic oxide, and in hydrogen, at reduced pressures, confirmed the results given in the first-mentioned paper, when the discharge was taken in the gases at atmospheric pressure. It was further shown that this peculiar spectrum could be reversed during the voltaic discharge in a lime crucible, provided magnesium and hydrogen were both present, but not in the absence of hydrogen. Likewise the flame of burning magnesium was found to emit this spectrum when the combustion occurred in an atmosphere containing either free or combined hydrogen. In summing up our results the following opinion was expressed:—

"The experiments above described, with nitrogen and carbonic oxide at reduced pressures, are almost if not quite conclusive against the supposition that the line at 5210 is due merely to the lower temperature of the spark in hydrogen. From De La Rue and Müller's observations it would appear that nitrogen at a pressure of 400 millims. should produce much the same effect on the spark as hydrogen at 760 millims. Now the pressures of the nitrogen and carbonic oxide were reduced far below this without any trace of the line in question being visible. Moreover, the magnesium line at 4481, which is not seen in the arc, and may be reasonably ascribed to the higher temperature of the spark, may be seen in the spark at the same time as the line at 5210 when hydrogen is present. Nevertheless, temperature does seem to affect the result in some degree, for when a large Leyden jar is used, and the gas is at the atmospheric pressure, the line almost disappears from the spark, to reappear when the pressure is reduced; but by no variation of temperature have we been able to see the line when hydrogen was carefully excluded.

"A line of the same wave-length has been seen by Young in the chromosphere once. Its absence from the Fraunhofer lines leads to the inference that the temperature of the sun is too high (unless at special times and places) for its production. If it be not due to a compound of magnesium with hydrogen, at any rate it occurs with special facility in the presence of hydrogen, and ought to occur in the sun if the temperature were not too high.

"We have been careful to ascribe this line and its attendant series to a mixture of magnesium and hydrogen rather than to a chemical compound, because this expresses the facts, and we have not yet obtained any independent evidence of the existence of any chemical compound of those elements."

Fig. 4 shows more completely than we have given it before the general character of this spectrum, which consists of two sets of flutings and a pair of fainter bands, the flutings closely resembling in character the hydrocarbon flutings, each fluting consisting of a multitude of fine lines closely set on the less refrangible side and becoming wider apart and weaker towards the more refrangible side, but extending under favourable circumstances much further than is shown in the figure. The set in the green is the stronger, and it was to this that our former observations were confined. It has two flutings, one beginning at about wave-length 5210 and the other close to b_1 on its more refrangible side. The other set consists of three principal flutings, of which the first begins at about wave-length 5618, the next at about wave-length 5566, and the third begins with three strong lines at about the wave-lengths 5513, 5512, 5511. Both sets are very well seen when a magnesium wire is burnt in the edge of a hydrogen flame, and in the arc in a crucible of magnesia when a gentle current of hydrogen is led into it. The less refrangible edges of the bands are at wave-lengths about 4849 and 4803.

As Mr. Lockyer, in a paper entitled "A New Method of Spectrum Observation" ("Proc. Roy. Soc.," vol. 30, p. 22) has brought forward this spectrum as illustrative and confirmatory of his views regarding the possibility of elemental dissociation at different heat-levels, we have been induced to review our former work. The view taken by Mr. Lockyer may be expressed in his own words.

"The flame spectrum of magnesium perhaps presents us best with the beautiful effects produced by the passage from the lower to the higher heat-level, and shows the important bearing on solar physics of the results obtained by this new method of work.

"In the flame the two least refrangible of the components of *b* are seen associated with a line less refrangible so as to form a triplet. A series of flutings and a line in the blue are also seen.

"On passing the spark all these but the two components of *b* are abolished. We get the wide triplet replaced by a narrow one of the same form, the two lines of *b* being common to both. . . . .

"May we consider the existence of these molecular states as forming a true basis for Dalton's law of multiple proportions? If so, then the metals in different chemical combinations will exist in different molecular groupings, and we shall be able, by spectrum observations, to determine the particular heat-level to which the molecular complexity of the solid metal, induced by chemical affinity, corresponds.

"*Examples.*—None of the lines of magnesium special to the flame spectrum are visible in the spectrum of the chloride either when a flame or a spark is employed."

In order to ascertain if this spectrum could be produced at a high temperature in the presence of hydrogen, which element we have already shown to be essential to its production at the atmospheric and at reduced pressures, the series of experiments already mentioned in describing the spark spectrum were made with hydrogen at pressures increasing up to twenty atmospheres.

On the supposition that this spectrum originates from the formation of some chemical compound, probably formed within certain limits of temperature when vapour of magnesium is in presence of hydrogen, the stability of the body ought to depend largely on the pressure of the gaseous medium. Like Graham's hydrogenium, this body might be formed at a temperature at which it would under ordinary circumstances be decomposed, provided the pressure of the hydrogen were correspondingly increased. In fact, it has been shown by Troost that the hydrides of palladium, sodium, and potassium all follow strictly the laws of chemical dissociation enunciated by Deville; and increased pressure by rendering the compound more stable, provided the secondary effect of such pressure in causing a higher temperature in the electric discharge were not overpowering, ought to conduce to a more continuous and brilliant spectrum of the compound. Conversely, if such a more continuous and brilliant spectrum be found to result, in spite of the higher temperature, from increased pressure, it can only be explained by the stability of the substance being increased with the pressure.

Now what are the facts? When the spark of an induction coil, without a Leyden jar, is passed between magnesium electrodes in hydrogen at atmospheric pressure, the flutings in the green are, as before described, always seen, but they are much stronger at the poles and do not always extend quite across the field. As the pressure is increased, however, they increase in brilliance and soon extend persistently from pole to pole, and go on increasing in intensity, until, at fifteen and twenty atmospheres, they are fully equal in brilliance to the *b* group, notwithstanding the increased brightness these have acquired by the higher temperature, due to the increased pressure. The second set of flutings, those in the yellowish-green, come out as the pressure is increased, and, in fact, at twenty atmospheres only the *b* group and the flutings are noticeable; if the yellow magnesium line be visible at all it is quite lost in the brilliance of the yellow flutings. The tail of fine lines of these flutings extend at the high pressure quite up to the green, and those of the green flutings quite up to the blue. On again letting down the pressure the like phenomena occur in the reverse order, but the brilliance of the flutings does not diminish so rapidly as it had increased. If, now, when the pressure has again reached that of the atmosphere, a large Leyden jar be interposed in the circuit, on passing the spark the flutings are still seen quite bright, and they continue to be seen with gradually diminishing intensity until the sparks have been continued for a considerable time. It appears that the compound, which had been formed in large quantity by the spark without jar at the higher pressures, is only gradually decomposed, and not re-formed, by the high temperature of the spark with jar. This experiment, which was several times repeated, is conclusive against the supposition that the flutings are merely due to a lower temperature. When the pressure was increased at the same time that the jar was employed, the flutings did not immediately disappear, but the expansion of the magnesium lines and the increase of the continuous spectrum seemed to overpower them.

When nitrogen was substituted for hydrogen, the strongest lines of the green flutings were seen when the spark without jar was first passed at atmospheric pressure, probably from hydrogen occluded, as it usually is, in the magnesium electrodes. As the pressure was increased they speedily disappeared entirely and were not again seen either at high or low pressures.

With carbonic oxide the same thing occurred as with nitrogen; but in this gas the flutings due to the oxide of magnesium (wave-length 4930 to 5000) were, for a time, very well seen.

Ciamician ("Sitzungsber. Akad. Wissensch.," Wien, 1880, p. 437) has described a spectrum of magnesium of the first order (in Plücker's nomenclature) obtained by taking sparks from an induction coil, without a jar, between magnesium electrodes in an atmosphere of hydrogen. He gives a figure to a scale of this spectrum, but it is not to a scale of wave-lengths, so that exact comparison of his observations with ours is difficult. The least refrangible set of flutings in his figure corresponds very well with that we have described in the yellowish-green. The next set, in the green, in his figure does not, however, correspond exactly with ours; it begins nearer to *b* than we have observed and consists of four flutings, whereas we observe but two in this set. It looks as if, in his figure, the magnesium-hydrogen spectrum were superposed upon the hydrocarbon spectrum in this region. Further, he gives a third more refrangible set of flutings which we have only observed as two blue bands, not fluted. This third set of flutings, as drawn in his figure, appears to be somewhat more refrangible than the set due to the oxide, and occupies partly the place of the blue hydrocarbon series, but a passage in the text, in which he says that the magnesium spectrum of the second order might, without measurement, easily be taken to be identical with that of carbon, almost negatives the supposition that this set of flutings is the blue hydrocarbon set and mistaken for a magnesium spectrum of the first order. To whatever it may be due, we have not seen anything closely resembling it under the circumstances described by him, though our observations on the spark spectrum of magnesium in hydrogen have now been repeated with all the variations of circumstance which we could devise.

Mr. Lockyer states (*loc. cit.*) that none of the lines of magnesium, special to the flame spectrum, are visible in the spectrum of the chloride, either when a flame or a spark is employed. But we find that when the spark is taken between platinum points from a solution of the chloride of magnesium, in a tube such as those used by Delachanal and Mermet, the line at wave-length 5210 can frequently be seen in it when the tube is filled with air, and that if the tube be filled with hydrogen the green flutings of magnesium-hydrogen are persistent and strong.

Repeated observations have confirmed our previous statements as to the facility with which the magnesium-hydrogen spectrum can be produced in the arc by the help of a current of the gas. In a magnesia crucible, by regulating the current of hydrogen, the flutings can be easily obtained either bright or reversed.

Comparison of the Spectra.

When we compare the spectra of magnesium in the flame, arc, and spark, we observe that the most persistent line is that at wave-length 2850, which is also the strongest in the flame and arc, and one of the strongest in the spark. The intensity of the radiation of magnesium at this wave-length is witnessed by the fact that this line is always reversed in the flame as well as in the arc when metallic magnesium is introduced into it, and in the spark between magnesium electrodes when a Leyden jar is used. It is equally remarkable for its power of expansion. In the flame it is a broad band, and equally so in the arc when magnesium is freshly introduced, but fines down to a narrow line as the metal evaporates.

Almost equal in persistence are the series of triplets. Only the least refrangible pair of these triplets is seen in the flame; another pair is seen in the spark, but the complete series is only seen in the arc. We regard the triplets as a series of harmonics, and to account for the whole series being seen only in the arc we must look to some other cause than the temperature. This will probably be found in the greater mass of the incandescent matter contained in the crucible in which the arc was observed.

The blue line of the flame at wave-length 4570 is well seen in the arc, and is easily reversed, but is always a sharp line, increased in brightness but not sensibly expanded by putting magnesium into the crucible. In the spark, at atmospheric pressure, it is only seen close to the pole or crossing the field in occasional flashes; but seems to come out more decidedly at rather higher pressures, at least in hydrogen.

The series of bands near L, well developed in the flame, but not seen at all in the arc or spark, look very much like the spectrum of a compound, but we have not been able to trace them to any particular combination. Sparks in air, nitrogen, and hydrogen have alike failed to produce them. The very strong, rather diffuse triplet at M, with which they end, so closely resembles in general character the other magnesium triplets, that it may well be connected with that constitution of the magnesian particle which gives rise to the triple sets of vibrations in other cases, but, if so, its presence in the flame alone is not easily explained.

The occurrence of this triplet in the ultra-violet, and of the remarkable series of bands associated with it, as well as the extraordinary intensity of the still more refrangible line at wave-length 2850, which is strongly reversed in the spectrum of the flame, corroborates what the discovery of the ultra-violet spectrum of water had revealed, that substances at the temperatures of flames while giving in the less refrangible part of the spectrum more or less continuous radiation, may still give, in the regions of shorter wave-length, highly discontinuous spectra, such as have formerly been deemed characteristic of the highest temperatures. This subject we will not discuss further at present, but simply remark what we have stated formerly, that "it opens up questions as to the enormous power for radiation of short wave-length of gaseous bodies at the comparatively low temperature of flame with regard to which we are accumulating facts."

In the arc and spark, but not in the flame, we have next a very striking group of two very strong lines at wave-lengths about 2801 and 2794, and a quintuple group of strong but sharp lines above them. The former are usually reversed in the spark with jar, and all are reversed in the arc when much magnesium is present. There are also several single lines in the visible part of the spectrum common to the arc and spark. All of these may be lines developed by the high temperature of the arc and spark. An indigo and a violet line in the arc have not been traced in the spark, but their non-appearance may be due to the same cause as that above suggested for the non-appearance of the higher triplets, the smallness of the incandescent mass in the spark.

A pair of lines in the arc near U appear to be represented in the spark by an equally strong, or stronger, pair near but not identical in position. The possibility of such a shift, affecting these two lines only in the whole spectrum and affecting them unequally, must in the present state of our knowledge be very much a matter of speculation. Perhaps sufficient attention has not hitherto been directed to the probability of vibrations being set up directly by the electric discharge independently of the secondary action of elevation of temperature. Some of the observations above described, and many others well known, indicate a selective action by which an electric discharge lights up certain kinds of matter in its path to the exclusion of others; and it is possible that in the case of vibrations which are not those most easily assumed by the particles of magnesium, the character of the impulse may slightly affect the period of vibration. The fact that, so far as observations go, the shift in the case of this pair of magnesium lines is definite and constant, militates against the supposition suggested. On the other hand, the ghost-like pairs of lines observed in the spark below the triplet near L, suggest the idea that some of the particles have their tones flattened by some such cause.

The strong pair at wave-length 2801, 2794, are accompanied in the spark, but not in the arc, by a much feebler, slightly more refrangible pair, but these have not the diffuse ghost-like character of those just alluded to.

These lines are phenomena of the high potential discharge in which particles are torn off the electrodes with great violence, and may well be thrown into a state of vibration which they will not assume by mere elevation of temperature.

There are two lines in the spark besides the well-known line at wave-length 4481 which have not been observed in the arc, but they are feeble and would be insignificant if it were not the fact that they, as well as the line at wave-length 4481, all short lines seen generally only about the poles, appear to be present in the solar spectrum. In the sun we seem to have all the lines common to the flame, arc, and spark (unless the line given in Ångström's map at 4570·9 be not identical as we believe it to be with the magnesium line), and possibly, judging by Rutherford's photograph, the strong triplet of the flame at M; but one line common

to the arc and spark at wave-length 4703 does not appear in Ångström's map. It is hard to account for its absence, as it is present in Kirchhoff's map and in Rutherford's photograph. We have noticed, however, that when the spark is taken in hydrogen, the line at wave-length 4570 appears stronger than that at wave-length 4703, while the reverse is the case when the atmosphere is nitrogen. It is possible then that the atmosphere may, besides the resistance it offers to the discharge, in some degree affect the vibrations of the metallic particles.

When we have made all the simplifications that we can by eliminating, as we hope we have done satisfactorily, the hydrogen-magnesium flutings, and by supposing the whole series of triplets to be harmonically related, and possibly some of the single lines also to be similarly related, we have still the fact that the chemical atoms of magnesium are either themselves capable of taking up a great variety of vibrations, or are capable by mutual action on each other, or on particles of matter of other kind, of giving rise to a great variety of vibrations of the luminiferous ether; and to trace satisfactorily the precise connexion between the occurrence of the various vibrations and the circumstances under which they occur, will require yet an extended series of observations.

IV. "Note on the Reduction of the Observations of the Spectra of 100 Sun-spots observed at Kensington." By J. NORMAN LOCKYER, F.R.S. Received May 12, 1881.

[PLATE 2.]

In anticipation of a more detailed communication, I beg to lay before the Royal Society some of the results of the reduction of the six most widened lines between F and *b* seen in the spectra of 100 sun-spots, observed at Kensington between November 12th, 1879, and September 29th, 1880, limiting my remarks solely to the spectrum of iron.

In the accompanying map, the Fraunhofer lines agreeing in position with the iron lines given by Ångström and Thalén are entered in the horizon headed "Sun," in the next are plotted the lines assigned to iron by Ångström, who used the electric arc in his experiments. In the next horizon are entered the iron lines given by Thalén, who employed the induction coil in his experiments. In these three horizons the lengths of the lines represent their intensities.

The individual observations of the sun-spots having been plotted out on another map, the number of times each line was seen was ascertained, and is entered in the next horizon under "Frequencies in Sun-Spots."

In the next horizon are entered the frequencies with which iron lines were seen in observations of a hundred flames by Tacchini. In both these horizons the lengths of the lines represent frequency.

This map shows that within the region F to *b*, there is no line common to spots and flames, if the lines b_3 b_4, which are so frequently seen affected in both spots and flames, be neglected. If, therefore, we were unacquainted with the spectrum of iron, we should be justified in saying that the spectrum of the prominences was due to one substance, and that of the spots to another.*

In a paper presented to the Royal Society in December, 1878, I drew attention to the fact that many of the lines most frequently seen in the flames by Young and Tacchini were assigned by Ångström to the spectra of two so-called elements. In the map I have entered his results in the horizon arc A.

I have since that time had prepared a table, showing the lines having coincident readings in two or more metals, according to Thalén, and in my paper of March 5th, 1879, "Discussion of Young's List of Chromospheric Lines," showed that some of these lines could not be due to impurity. The lines given coincident by Thalén in his tables, are entered in the horizon spark T.

I have confirmed in most cases Thalén's and Ångström's work, and have proved that these lines could not be due to an impurity of the one in the other, as the longest lines of each were absent from the other. I used the arc, quantity coil, and intensity coil, which are respectively indicated by arc L, spark LQ, and LI, with high dispersion.

Besides confirming Thalén's and Ångström's work, I have been able to add a few more basic lines to the list.

I have already pointed out that the fact of different rates of motion being indicated by different iron lines in the same field of view at the same time, afforded important evidence that we were not dealing with iron itself, but with primitive forms of matter contained in iron, which are capable of withstanding the high temperature of the sun, after the iron, observed as such, has been broken up as suggested by Brodie.†

An appeal to the principles of continuity and evolution now enables us to add another argument of equal weight. If on the cooling of a

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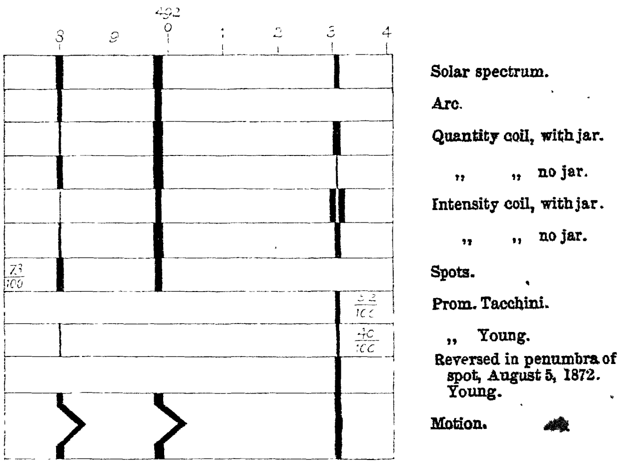
* It will be observed that the line at 5017·5 given among the flame observations in the map is not recorded as an iron line by either Ångström or Thalén. The way, however, in which it sympathised with the line at 4923 in the flames induced me to look for it in iron with the intensity coil. I at once found it, and it is as sympathetic with the line at 4923 in the spark as it is in the flames themselves. Though omitted by Thalén (most likely accidentally, though it may well be that he did not use sufficient tension to bring it out strongly), it is recorded as an iron line by other observers.

† Or rather, of course, before "iron" had been formed by condensation.

mass of vapour ultimately to form a star, each reduction of temperature increases the number and complexity of the chemical forms by rendering new combinations possible, these new combinations will contain the earlier ones in different proportions. If, for instance, the members of the iron groups are not elementary, they will contain earlier forms, as the salts of calcium contain calcium, which once existed as calcium in the atmosphere of the earth before the salts were produced as the result of a subsequent condensation brought about by cooling.

The discussion in the accompanying map shows that this is really the case, and that it is the lines which are common to two or more substances which in the main produce the spectra of the lower, and therefore hotter, region of the solar atmosphere. This natural result at once explains the strange variations from the ordinary spectra which have puzzled observers ever since the new method was introduced. More than this, these are precisely those lines which have their intensities strengthened when we pass from the arc to the coil.

It is very instructive to note the gradual simplification of the iron spectrum by increased temperature as we pass from the arc through the spots to the flames, and how with this increasing simplicity we find the basic character of the lines increasing, only one basic line on the map having been yet observed among those lines seen in the arc alone.



The accompanying figure shows what happens with regard to three adjacent iron lines under different solar and terrestrial conditions, and I give it because it indicates that one of the most important inquiries to be taken up in the eclipses of next year and 1883, will be the determination of the intensities of the spot and flame lines, and the heights in the solar atmosphere at which they are visible at the moment of disappearance and reappearance of the sun. The spectrum of the lower parts of the corona itself should also be observed, with a proper amount of dispersion with instruments affording a great quantity of light.

I stated some time ago to the Royal Society that the iron lines visible in the arc but not in the spark, in all probability owe their existence among the Fraunhofer lines to an absorption going on in the cooler levels, above the flame and spot regions. A look at the map will show how important it will be to adjust a spectroscope on the lines shown in the figure, and contrast their behaviour with the other twelve adjacent arc lines which are prominent neither in spark nor spot, nor flame. It is to be feared that any observer who attempts to do more than such a restricted piece of work as this will doubt his own results. The time has arrived when minute observations must take the place of mere general ones.

May 19, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The Right Hon. William Ewart Gladstone was admitted into the Society.

The following Papers were read:—

I. "On Discontinuous Phosphorescent Spectra in High Vacua." By WILLIAM CROOKES, F.R.S. Received March 31, 1881.

In a paper which I had the honour of presenting to the Royal Society in March, 1879,* I drew attention to the fact that many substances, when in high vacua and submitted to the molecular discharge by means of an induction coil, emitted phosphorescent light; and I especially mentioned the phosphorescent sulphides, the diamond, the ruby, and various other forms of alumina, crystalline and amorphous.

Pure alumina chemically prepared has very strong phosphorescence. Sulphate of alumina is dissolved in water, and to it is added an excess of solution of ammonia. The precipitated hydrate of alumina is filtered, washed, ignited, and tested in the molecular stream. It phosphoresces of the same crimson colour, and gives the same spectrum as the ruby.

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* "Phil. Trans.," vol. 170.

Alumina in the form of ruby glows with a full rich red colour, and when examined in the spectroscope the emitted light is seen to be discontinuous. There is a faint continuous spectrum ending in the red somewhere near the line B; then a black space, and next an intensely brilliant and sharp red line, to which nearly the whole of the intensity of the coloured glow is due. The wave-length of this red line, which appears characteristic of this form of alumina, is, as near as I can measure, λ 689·5 m.m.m. This line coincides with the one described by E. Becquerel as being the most brilliant of the lines in the spectrum of the light of alumina in its various forms, when glowing in the phosphoroscope.

This coincidence is of considerable interest, as it shows a relation between the action of molecular impact and of sunlight in producing luminosity. The phosphorescence induced in a crystal of ruby by the molecular discharge is not superficial, but the light comes from the interior of the crystal, and is profoundly modified according as its direction of vibration corresponds or makes an angle with the axis of the crystal, being quenched in certain directions by a Nicol prism.

Sunlight falling on the ruby crystal produces the same optical phenomena. The light is internally emitted, and on analysis by a prism, is seen to consist essentially of the one brilliant crimson line, λ 689·5. This fact may account for the extraordinary brilliancy of the ruby, which makes it so highly prized as a gem. The sun not merely renders the red-coloured stone visible, as it would a piece of coral, but it excites the crystal to phosphorescence, and causes it to glow with a luminous internal light, the energy of which is not diffused over a broad portion of the spectrum, but is chiefly concentrated into one wave-length.

The crimson glow of alumina remains visible some time after the current ceases to pass. When the residual glow has ceased it can be revived by heating slightly with a spirit-lamp.

After long experimenting with chemically pure alumina precipitated from the sulphate as above described, a curious phenemenon takes place. When sealed up in the vacuum two years ago it was snow white; but, after being frequently submitted to the molecular discharge for the purpose of exhibiting its brilliant phosphorescence, it gradually assumes a pink tinge, and on examination in sunlight a trace of the alumina line can be detected. The repeated molecular excitation is slowly causing the amorphous powder to assume a crystalline form.

Under some circumstances, alumina glows with a green colour. Ammonia in large excess was added to a dilute solution of alum. The strong ammoniacal solution filtered from the precipitated alumina was now boiled. The alumina which the excess of ammonia had dissolved was thereby precipitated. This was filtered off, ignited, and tested in the molecular discharge. It gave no red light whatever, but phosphoresced of a pale green, and on examination with a prism the light showed no lines, but only a concentration of light in the green.

Two earthen crucibles were tightly packed, the one with sulphate of alumina, the other with acetate of alumina. They were then exposed, side by side, to the most intense heat of a wind furnace —a heat little short of the melting-point of platinum.* The resulting aluminas were then tested in the molecular stream.

The alumina from the sulphate gave the crimson glow and the spectrum line.

The alumina from the acetate gave no red glow or line, but a pale green phosphorescence.

In my examination of rubies, many pounds of which have passed through my apparatus, I have been fortunate enough to meet with one solitary crystal, not to the eye different from others, which emits a *green* light when tested in the molecular stream. All others act as I may call normally. The spectrum of this green-glowing crystal shows, however, a trace of the red line, and on keeping the discharge acting on it for a few minutes the green phosphorescence grows fainter and a red tinge is developed, the spectrum line in the red becoming more distinct.

Besides the ruby, other native forms of crystallised alumina phosphoresce. Thus corundum glows with a pink colour. The sapphire appears to be made up of the red-glow and green-glow alumina. Some fine crystals of sapphire shine with alternate bands of red and green, arranged in layers perpendicular to the axis. Unfortunately it is impossible to prepare a tube for exhibition containing this variety of sapphire, as it is constantly evolving gas from the numerous fissures and cavities which abound in this mineral.

The red glow of alumina is chiefly characteristic of this earth in a free state. Few of its compounds, except spinel (aluminate of magnesium), either natural or artificial, show it in any marked degree. All the artificially crystallised aluminas give a strong red glow and spectrum line. An artificially crystallised aluminium and barium fluoride phosphoresces with a blue colour, but shows the red alumina line in the spectrum. Spinel glows red, and gives the red line almost as strong as the ruby.

The mineral spodumene (an aluminium and lithium silicate) phosphoresces very brilliantly with a rich golden-yellow colour, but shows no spectrum line, only a strong concentration of light in the orange and yellow. A phosphorescing crystal of spodumene has all the internal light cut off with a Nicol prism, when the long axes of the Nicol and the crystal are parallel.

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* This operation was kindly performed for me by Messrs. Johnson and Matthey.

It became of interest to see if the other earths would show phosphorescent properties similar to those of alumina, and especially if any of them would give a discontinuous spectrum; considerable interest attaching to a solid body whose molecules vibrate in a few directions only, giving rise to spectrum lines or bands on a dark background.

Glucina prepared with great care is found to phosphoresce with a bright blue colour, but no lines can be detected in the spectrum, only a concentration of light in the blue.

The rare mineral phenakite (silicate of glucinum), sometimes used as a gem, phosphoresces blue like pure glucina. This mineral shows a residual glow after the current is turned off.

Thorina has very little, if any, phosphorescence. This earth is, however, remarkable for its very strong attraction for the residual gas in the vacuum tube. On putting thorina in a tube furnished with well-insulated poles, whose ends are about a millimetre apart in the centre, and heating strongly during exhaustion, the earth, on cooling, absorbs the residual gas with such avidity that the tube becomes non-conducting, the spark preferring to pass several inches in air rather than strike across the space of a millimetre separating the two poles. It is probable that this strong attraction for gas is connected with the great density of the earth thorina (sp. gr. = 9·4).

Zirconia gives a very brilliant phosphorescence, approaching in intensity that of sulphide of calcium. The colour is pale bluish-green, becoming whiter as the intensity of the discharge increases: no lines are seen in its spectrum.

Lanthana precipitated as hydrate and ignited shows no phosphorescence. After it has been heated for some time before the blowpipe it phosphoresces of a rich brown.

Didymia, from the ignition of the hydrate, has scarcely any phosphorescence; what little there is appears to give a continuous spectrum with a broad black band in the yellow-green. On examining the light reflected from this earth when illuminated by day or artificial light, the same black band is seen, and with a narrow slit and sunlight the band is resolved into a series of fine lines, occupying the position of the broadest group of absorption lines in the transmission spectrum of didymium salts.

Yttria shows a dull greenish light, giving a continuous spectrum.

Erbia phosphoresces with a yellowish colour, and gives a continuous spectrum, with the two sharp black bands so characteristic of this earth cutting through the green at λ 520 and 523. These lines are easily seen in the light reflected from erbia when illuminated by daylight. It is well known that solid erbia heated in a flame glows with a green light, and gives a spectrum which chiefly consists of two bright green lines in the same place as the dark lines seen by reflected light.

A curious phenomenon is presented by erbia when the spark passes over it at a high exhaustion. The particles of earth which have accidentally covered the poles are shot off with great velocity, forming brightly luminous lines, and, striking on the sides of the tube, rebound, remaining red-hot for an appreciable time after they have lost their velocity. They form a very good visible illustration of radiant matter.

Titanic acid phosphoresces dark brown, with gold spots in places.

Stannic acid gives no phosphorescence.

Chromic, ferric, and ceric oxides do not appreciably phosphoresce.

Magnesia phosphoresces with a pink opalescent colour, and shows no spectrum lines.

Baryta (anhydrous) scarcely phosphoresces at all. Hydrated baryta, on the contrary, shines with a bright orange-yellow light, but shows no discontinuity of spectrum; only a concentration in the yellow-orange.

Strontia (hydrated) phosphoresces with a beautiful deep blue colour, and when examined in the spectroscope the emitted light shows a greatly increased intensity at the blue and violet end, without any lines or bands.

Lime phosphoresces of a bright orange-yellow colour, changing to opal-blue in patches where the molecular discharge raises the temperature. In the focus of a concave pole the lime becomes red and white-hot, giving out much light. This earth commences to phosphoresce more than 5 millims. below the vacuum, and continues to grow brighter as long as the electricity is able to pass through the tube. On stopping the discharge there is a decided residual glow. No lines are seen in the spectrum of the light.

Calcium carbonate (calcite) shows a strong phosphorescence, which begins to appear at a comparatively low exhaustion (5 millims.). The interior of the crystal shines of a bright straw colour, and the ordinary and extraordinary rays are luminous with oppositely polarised light. Calcite shows the residual glow longer than any substance I have as yet experimented with. After the current has been turned off the crystals shine in the dark with a yellow light for more than a minute.

Calcium phosphate generally gives an orange-yellow phosphorescence and a continuous spectrum. Sometimes, however, a yellow-green band is seen superposed on the spectrum.

Potash phosphoresces faintly of a blue colour. The spectrum shows a concentration at the blue end, but the light is too faint to enable lines, if any, to be detected.

Soda phosphoresces faintly yellow, and gives the yellow line in the spectrum.

Lithium carbonate gives a faint red phosphorescence. Examined in the spectroscope, the red, orange, and blue lithium lines are seen.

I have already said that the diamond phosphoresces with great brilliancy. In this respect perfectly clear and colourless stones "of the first water" are not the most striking, and they generally glow of a blue colour. Diamonds, which in sunlight have a slight fluorescence disappearing when yellow glass is interposed, generally phosphoresce stronger than others, and the emitted light is of a pale yellowish-green colour.

Most diamonds which emit a very strong yellowish light in the molecular discharge give a continuous spectrum, having bright lines across it in the green and blue. A faint green line is seen at about λ 537; at λ 513 a bright greenish-blue line is seen, and a bright blue line at λ 503. A darkish space separating the last two lines.

Diamonds which phosphoresce red generally show the yellow sodium line superposed on a continuous spectrum.

There is great difference in the degree of exhaustion at which various substances begin to phosphoresce. Some refuse to glow until the exhaustion is so great that the vacuum is nearly non-conducting, whilst others commence to become luminous when the gauge is 5 or 10 millims. below the barometric level. The majority of bodies, however, do not phosphoresce till they are well within the negative dark space.

During the analysis of some minerals containing the rarer earths experimented on, certain anomalies have been met with, which seem to indicate the possible presence of other unknown elements awaiting detection. On several occasions an earthy precipitate has come down where, chemically speaking, no such body was expected; or, by fractional precipitation and solution, from a supposed simple earth something has separated which, in its chemical characters, was not quite identical with the larger portion; or, the chemical characteristics of an earth have agreed fairly well with those assigned to it in books, but it deviated in some physical peculiarity. It has been my practice to submit all these anomalous bodies to molecular bombardment, and I have had the satisfaction of discovering a class of earthy bodies which, whilst they phosphoresce strongly, also give spectra of remarkable beauty.

The spectrum seen most frequently is given by a pale yellowish coloured earth. It consists of a red, orange, citron, and green band, nearly equidistant, the citron being broader than the others and very bright. Then comes a faint blue, and lastly two very strong blue-violet bands. These bands, when seen at their best, are on a perfectly black background; but the parent earth gives a continuous spectrum, and it is only occasionally, and as it were by accident, that I have so entirely separated it from the anomalous earth as to see the bands in their full purity. Another earthy body gives a spectrum similar to that just described, but wanting the red, and having a double orange and double citron band. A third gives a similar spectrum, but with a yellow line interposed between the double orange and the double citron, and having two narrow green lines.

At present I do not wish to say more than that I have strong indications that one, or perhaps several new elements are here giving signs of their existence. The quantities I have to work upon are very small, and when each step in the chemical operation has to be checked by an appeal to the vacuum tube and to the induction coil the progress is tediously slow. In the thallium research it only occupied a few minutes to take a portion of a precipitate on a platinum loop, introduce it into a spirit-flame, and look in the spectroscope for the green line. In that way the chemical behaviour of the new element with reagents could be ascertained with rapidity, and a scheme could be promptly devised for its separation from accompanying impurities. Here, however, the case is different: to perform a spectrum test, the body under examination must be put in a tube and exhausted to a very high point before the spectroscope can be brought to bear on it. Instead of two minutes, half a day is occupied in each operation, and the tentative gropings in the dark, unavoidable in such researches, must be extended over a long period of time.

The chemist must also be on his guard against certain pitfalls which catch the unwary. I allude to the profound modification which the presence of fluorine, phosphorus, boron, &c., causes in the chemical reactions of many elements, and to the interfering action of a large quantity of one body on the chemical properties of another which may be present in small quantities.

The fact of giving a discontinuous phosphorescent spectrum is in itself quite insufficient to establish the existence of a new body. At present it can only be employed as a useful test to supplement chemical research. When, however, I find that the same spectrum-forming earthy body can always be obtained by submitting the mineral to a certain chemical treatment; when the chemical actions which have separated this anomalous earth are such that only a limited number of elements can possibly be present; when I find it impossible to produce a substance giving a similar discontinuous spectrum by mixing together any or all of the bodies which alone could survive the aforesaid chemical treatment; when all these facts are taken into consideration, and when due weight is given to the very characteristic spectrum reaction, I cannot help concluding that the most probable explanation is that these anomalies are caused by the presence of an unknown body whose chemical reactions are not sufficiently marked to have enabled chemists to differentiate it from associated elements.

III. "Molecular Magnetism." By Professor D. E. HUGHES, F.R.S. Received May 10, 1881.

During the course of some late researches, which I had the honour to communicate to the Royal Society, March 7th,* and experimentally illustrate on the reading of my second paper, March 31,† many experimental facts occurred, all pointing to the conclusion that ordinary molar magnetism is entirely due to the symmetrical arrangement of the polarised molecules, and that these molecules can be rotated by torsion, so as to decrease the longitudinal magnetism, or increase it, if the effect of the elastic torsion is to rotate the molecules into the required longitudinal symmetrical arrangement. And observing that molecular magnetism could induce an electric current upon its own molar constituents, or that an electric current by its passage through an iron wire would produce molecular magnetism, I have continued these researches in the hope of elucidating, as far as possible, the phenomenon of the transformation of electricity and magnetism by the changes produced in the molecular structure of its conducting wire.

For this purpose I have employed three separate methods of investigation, each requiring a slightly modified form of apparatus. The first relates to the influence of an elastic torsion upon a magnetic or conducting wire; the second, to the influence upon the molecular structure of an iron wire of electricity or magnetism; the third, to the evident movement of the molecules themselves as given out in sonorous vibrations.

The general details of the apparatus employed having been given in my paper of March 7th, I will only briefly indicate any modification of the method employed.

1. *Influence of an Elastic Torsion upon a Magnetic or an Electric Conducting Wire.*

In my paper of March 7th on "Molecular Electro-Magnetic Induction," I showed that induced currents of electricity would be induced in an iron wire placed on the axis of a coil through which intermittent currents were passing, and that these currents were produced only when the wire was under the influence of a torsion not passing its limit of elasticity. It became evident that if the intermittent magnetism induced by the coil produced under torsion intermittent currents of electricity, an intermittent torsion under the influence of a constant current of electricity or a constant magnetic field would produce similar currents. This was found to be the case, and as some new phenomena presented themselves indicating clearly the molecular nature of the actions, I will describe a few of them directly relating to the subject of this paper.

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* "Molecular Electro-Magnetic Induction," "Proc. Roy. Soc.," vol. 31, p. 525.

† "Permanent Molecular Torsion of Conducting Wires produced by the Passage of an Electric Current," *Ibid.*, vol, 32, p. 25.

The apparatus used was similar to that described in my paper of March 7th. An iron wire of 20 centims. was placed in the centre or axis of a coil of silk-covered copper wire, the exterior diameter of the coil being 5½ centims., and that of the interior vacant circular space 3½ centims. The iron wire is fastened to a support at one end, the other passing through a guide, to keep it parallel to the axis but free, so that any required torsion may be given to the wire by means of a connecting arm or index. A sensitive telephone is in direct communication with the coil, or a galvanometer may be used, as the currents obtained by a slow elastic torsion are slow and strong enough to be seen on a very ordinary galvanometer. I prefer, however, the telephone, because it has the inestimable advantage in these experiments of giving the exact time of the commencement or end of an electric current. It has, however, the disadvantage of not indicating the force or direction of the current; but by means of the sonometer the true value and direction of any current is at once given. Again, the telephone is useless for currents of slow intermittence; but, by joining to it the microphonic rheostat described in my paper of March 7th, a slowly intermittent or permanent current is broken up into rapidly intermittent currents, and then we are able to perceive feeble constant currents. For this reason a microphonic rheostat is joined to the telephone and coil. The current from a battery of two bichromate cells is sent constantly through the wire if we wish to observe the influence of the torsion of the wire upon the electric current, or a constant field of magnetic energy is given to the wire by either a separate coil or a permanent magnet. The currents obtained in the coil are induced from the change in the molecular magnetism of the wire, but we may equally obtain these currents in the wire itself without any coil by joining the telephone and rheotome direct to the wire; in the latter case, it is preferable to join the wire to the primary of a small induction coil, and the telephone and rheotome to the secondary, as then the rheotome does not interrupt the constant electric current passing through the wire. As the results are identical, I prefer to place the telephone on the coil first named, as the tones are louder and entirely free from errors of experimentation.

If we place a copper wire in the axis of the coil we produce no effect by torsion, either when under the influence of a constant magnetic field or a current passing through it, nor do we perceive any effects if we place an iron wire (2 millims. in diameter), entirely free from magnetism and through which an electric current has never passed. I mention this negative experiment in order to prove that all the effects I shall mention are obtained only through the magnetism of the wire. If now I pass an electric current for an instant through this same wire, its molecules are instantly polarised. I have never yet been able to restore the wire to its original condition, and the magnetisation induced by the passage of a current is far more powerful and more persistent in soft iron than tempered steel. This may be due, however, to the fact that in tempered or softened steel we find traces only of a current during to the rotation by torsion of its molecules, some two to three degrees of sonometer, whilst iron gives constantly a current of 70 sonometric degrees.*

In order to obtain these currents, we must give a slight torsion of 5° or 10° to and fro through the zero point. We then have a current during the motion of the index to the right, and a contrary current in moving the index to the left. If we use a galvanometer, we must time these movements with the oscillations of the needle; but with the telephone it gives out continuous sounds for either movement, the interruptions being only those caused by the rheotome. The direction of the current has no influence on the result; either positive to the free arm or index or negative gives equal sounds, but at the moment of reversal of the current a peculiar loud click is heard, due to the rapid change or rotation of the polarisation of its molecules, and this peculiarly loud momentary click is heard equally as well in steel as in iron, proving that it is equally polarised by the current, but that its molecular rigidity prevents rotation by torsion. We can imitate in some degree the rigidity of steel by giving the iron wire several permanent twists. The current due to elastic torsion is then reduced from 70° to 40°, in consequence of the mechanical strain of the twists remaining a constant; and a weakening of the current is also remarked if with a fresh wire we pass in torsion its limit of elasticity.

If a new soft iron wire of 2 millims. (giving no traces of a current by torsion) has passed through it a momentary current of electricity, and the wire is then observed free from the current itself, it will be found to be almost as strongly polarised as when the current was constantly on, giving by torsion a constant of 50 sonometric degrees. If, instead of passing a current through this new wire, I magnetise it strongly by a permanent magnet or coil, the longitudinal magnetism gives also 70° of current for the first torsion, but weakens rapidly, so that in a few contrary torsions only traces of a current remain, and we find also its longitudinal magnetism almost entirely dissipated. Thus there is this remarkable difference, that, whilst it is almost impossible to free the wire from the influence produced by a current, the longitudinal magnetism yields at once to a few torsions. We may, however, transform the ring or transversal magnetism into longitudinal magnetism by strongly magnetising the wire after a current has passed through it. This has the effect of rotating the whole of the molecules, and they are all now symmetrical with longitudinal magnetism; then, by a few torsions, the wire is almost as free as a new wire. I have found this method more efficacious than heating the wire red hot, or any other method yet tried. If I desire a constant current from longitudinal magnetism, I place at one of the extremities of the wire a large permanent magnet, whose sustaining power is 5 kilogrammes, and this keeps the wire constantly charged, resembling in some respects the effects of a constant current. The molecular magnetism or the current obtained by torsion is not so powerful from this, my strongest magnet, as that produced by the simple passage of a current, being only 50 sonometric degrees in place of 70° for that due to the passage of a current. The mere twisting of a longitudinal magnet, without regard to the rotation of its molecules has no effect, as is proved by giving torsion to a steel wire strongly magnetised, when only traces of a current will be seen, perhaps one or two degrees, and by using a constant source of magnetism or electricity, when no measurable effect will be obtained. Evidently we have as much twisted the magnetised steel as the soft iron. In the steel we have a powerful magnet, in the soft iron a very feeble one; still the molecular rotation in iron produces powerful currents to the almost absolute zero of tempered steel.*

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* 0·8 of a Daniell battery.

If we magnetise the wire whilst the current is passing, and keep the wire constantly charged with both magnetism and electricity, the currents are at once diminished from 70° to 30°. We have here two distinct magnetic polarisations at right angles to each other, and no matter what pole of the magnet, or of the current, the effect is greatly diminished; the rotation of the two polarities would now require a far greater arc than previously. The importance of this experiment cannot as yet be appreciated until we learn the great molecular change which has really occurred, and which we observe here by simply diminished effects.

If we heat the wire with a spirit flame, we find the sounds increase rapidly from 70 to 90, being the maximum slightly below red heat. I have already remarked in my previous paper this increased molecular activity due to heat, and its effects will be more clearly demonstrated when we deal with the sounds produced by intermittent currents.

Another method, by means of which I have again received proofs of the rotation of the polarised molecules, is to pass an intermittent current through a soft 0·5 millim. iron wire, listening to the results by the telephone joined direct and alone to the coil, as described in my paper of March 7. If the wire is then entirely free from strain, we have silence, but a torsion of 20° produces some 50 sonometric degrees of electric force. If, now (the wire being at zero strain), I bring one pole of the permanent magnet I have already described near the side of the wire, the sounds increase from zero up to 50°, being at their maximum when this magnet is 5 centims. distant; but if we continue to approach the magnet the sounds gradually weaken almost to zero. The explanation of this fact can be found when we know that the greatest inductive effect on the wire would be when a magnet is at an angle of 45° with the wire. And, also, considering each molecule as a separate independent magnet, we find that at a given distance for a given magnet the force of rotation is equal to that of 45°; by approaching the magnet we increase the rotation but diminish the angular polarity in relation to the wire, hence the decrease of force by the near approach of the magnet. And to prove that the function of the elastic torsion is simply to rotate the polarised molecules similarly to the magnet, we place the wire under an elastic torsion of 20°, and approach gradually the magnet as before. One pole now will be found to increase the sounds or its angular polarity, the other will diminish them, until at 5 centims. distance, as before, we have perfect silence; the torsion exists as before, but the molecules are no longer at the same angle. On removing the magnet we find that instead of the usual 50 of current we obtain barely 5 or 10: have we then destroyed the polarity of the molecules, or do they find a certain resistance to their free rotation to their usual place? To solve this question we have only to shake the wire, or give it a slight mechanical vibration, and then instantly the molecules rotate more freely, and we at once find our original current of 50°. I will forbear mentioning many other experimental proofs of my views by this method, as there are many to relate by different methods in the following sections.

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* I purposely avoid using the terms "magnetic fluid" and "coercitive force."

2. *Influence upon the Molecular Structure of an Iron or Steel Wire of Electricity or Magnetism.*

Being desirous to modify the apparatus already described, so that it should only give indications of a current if it were of a spiral nature, the wire was kept rigidly at its zero of strain or torsion, and the coil was made so that it could revolve on an axis perpendicular to the wire; by this means, if the wire was free from strain, the centre or axis of the coil would coincide with that of the wire. Thus, with a straight copper wire, we should have a complete zero, but if this wire formed a right or left-handed helix, the coil would require moving through a given degree (on an arbitrary scale) corresponding to the diameter and closeness of the spirals in the helix; the degrees through which the coil moved, were calibrated in reference to known copper helices. 50° equalled a copper wire 1 millim. diameter, formed into a helix of 1 centim. diameter, whose spiral turns were separated 1 centim. apart.

In order to obtain a perfect zero, and wide readings, with small angular movement of the coil, it is necessary that the return wire should be of copper, 2 millims. diameter, offering comparatively little resistance, and that it should be perfectly parallel with the steel or iron wire. In order that it may react upon the exterior of the coil, it is fastened to the board, so that it is near (1 centim.) the exterior of the coil, and parallel to the iron wire, at a distance of 4 centims. If we consider this return wire alone, we find, as in the sonometer, that if the wire is perpendicular to the exterior wires of the coil, we have a zero or silence, but moved through any degree, we have a current proportionable to that degree; by this means, we have an independent constant acting on the coil, constantly aiding the coil in finding its true zero, and allowing of very wide readings, with a comparatively small angular movement of the coil.

The rheotome is joined to a battery of two bichromate cells, and by means of a reversing switch, an intermittent current of either direction can be sent through the wire. The telephone is joined direct and alone to the coil; thus no currents react upon the coil when perpendicular to the iron, and its return wire, if not of a spiral nature.

Placing an iron wire 0·5 diameter, and passing a current through it, I found a change had taken place similar to those indicated in my paper of March 17th; but it was so difficult to keep the wire free from magnetism and slight molecular strains, that I preferred and used only in the following experiments tempered steel wire (knitting needles I found most useful). All the effects are greatly augmented by the use of iron wire, but its molecular elasticity is so great that we cannot preserve the same zero of reading for a few seconds together, whilst with steel, 0·5 millim. diameter, the effects remained constant until we removed the cause.

I have not as yet been able to obtain a steel wire entirely free from magnetism, and as magnetism in steel has a remarkable power over the direction of the spiral currents, I will first consider those in which I found only traces. On passing the intermittent current through these, the sounds were excessively feeble for either polarity of current, but, at each reversal, a single loud click could be heard, showing the instant reversal of the molecular polarity. The degree of coil indicating the twist or spirality of the current was 5° on each side of its true zero. The wire was now carefully magnetised, giving 10° on each side for different currents. The positive entering at north pole indicating 10° right-handed spiral, the negative entering the same pole, a left-handed spiral, we here see in another form, a fact well known and demonstrated by De la Rive by a different method, that an electric current travels in spirals around a longitudinal magnet, and that the direction of this spiral is entirely due to which pole of an electric current enters the north or south pole. I propose soon, however, to show that under certain conditions these effects are entirely reversed.

If through this magnetised wire I pass a constant current of two bichromate cells, and at the same time an intermittent one, the spiral is increased to 15°, but the direction of the intermittent current entirely depends on that of the constant current. Thus, if the positive of the constant current enters the north pole, the intermittent positive slightly increases the spiral to 17°, and the negative to 13°, both being right-handed; the two zeros of the constant battery are, however, as we might expect from the preceding experiment, on opposite sides of and at equal distances from the true zero; but if we magnetise the wire whilst a constant current is passing through it, a very great molecular disturbance takes place; loud sounds are heard in the telephone, and it requires for each current a movement of the coil of 40°, or a total for the two currents of 80°. This, however, is not the only change that has taken place, as we now find that both constant currents have a right-handed spiral; the positive under which it was magnetised, a right-handed spiral of 95°; the negative, a right-handed spiral of 15°, and the true central or zero point of the true currents indicates a permanent spiral of 55°.

This wire was magnetised in the usual way, by drawing the north pole of my magnet from the centre to one extremity, the south from the centre to the other, and this was repeated until its maximum effects were obtained. In this state I found, sliding the coil at different portions, that the spiral currents were equal, and in the same direction throughout.

It now occurred to me to try the effect of using a single pole of the magnet; this was done whilst a constant current was passing through the wire, commencing at the extremity where the positive joined, drawing the north pole through the length of the wire, from positive towards the negative; the effect was most remarkable, as the steel wire now gave out as loud tones as a piece of iron, and the degree on the coil showed 200°. The constant and intermittent currents now showed for either polarity a remarkably strong right-handed twist; the positive 200 right, and the negative 150 right-handed spirals. The molecular strain on the wire from the reaction of the electric current upon the molecular magnetism was so great, that no perfect zero would be obtained at any point, a fact already observed when a wire was under an intense strain, producing tertiary currents that superposed themselves upon the secondary. In order to compare these spiral currents with those obtained from a known helix, I found that taking a copper wire of similar diameter (0·5 millim.), and winding it closely upon the steel wire ten turns to each centimetre, having a total of 200 turns, with an exterior diameter of 1·5 millims., withdrawing the steel wire, leaving this closely wound helix free, it gave some 190°, instead of the 200 of the steel wire alone; thus the spiral currents fully equalled a closely wound copper wire helix of 200 turns in a similar length.

If it were possible to twist a magnetised wire several turns to the right, and that its line of magnetism should coincide with that of the twist, then on passing a positive or negative current, there would be an apparent augmented or diminished spirality of the current, but both would have a right-handed twist. The result would be identical with the phenomenon described, although the cause is different.

The explanation of this phenomenon can be probably found in the fact that the constant spirality now observed is that of the electric current under which it was magnetised, for whilst magnetising it we had a powerful source of magnetism constantly reacting upon the electric current, and the constant spirality now observed is the result or remains of a violent molecular reaction at the instant of magnetisation, and the remaining evident path or spiral is that of the electric current. On testing this wire as to its longitudinal magnetic force, I found that it was less than that of a wire simply magnetised in the usual way; thus the effects are internal, affecting the passage of the electric current, giving, however, no external indications (except apparent weakness) of the enormous disturbance which has taken place.

If, instead of drawing the north pole of the magnet as above, from positive towards negative, I draw it from negative to positive, all the effects are repeated, except that we have now, as we should expect, a left-handed spiral. But if I draw the magnet from the extremities of the wire to the centre, then at this centre I find an absolute zero of twist, but on each side a contrary twist, the wire then having a left and right-handed twist, the positive travelling towards the centre in a right-handed twist gradually ceasing in zero; this is as we might expect, but if done under the influence of a constant current, no matter what pole of the battery enters afterwards the north pole of the magnet, it will have during its first half a right-handed, and its second a left-handed spiral. It became important to know if a wire which had been magnetised under the influence of a current could be restored to something like its original condition. Electric currents had no effect. Heat, which would not destroy its temper, had no effect. Mechanical vibrations and torsions failed to disturb the molecular arrangement; but magnetising it strongly by a magnet, when no current was passing, at once brought the wire to its usual apparently rigid state, and the constant or intermittent currents now indicated only 18° of spiral currents against a previous 200°, and the sounds were, as usual from steel, excessively weak. I have since used this method with invariable success, when I wished to repeat the experiments upon the same wire. If these experiments are repeated upon an iron wire, the effects are far greater in the first instance, so great that they were thrown out of the range of my measurements; it was only after a few seconds of successive reversals that the zero of the wire was brought within range, and although these rapidly decreased, exactly similar effects were observed as in the steel. And as with all moderate ranges, I could bring the iron at once to a complete zero by torsion, and as torsion alone would produce this complete zero, I believe we have here effects from causes identical with those related in the first chapter.

Having noticed in my previous papers the increased molecular activity caused by the approach of a powerful permanent magnet, and believing that the permanent spirality above mentioned was due to this alone, and not to an increased polarity, I magnetised strongly an iron wire giving as usual a reversed spiral for different currents of but 10°. I now heated the wire by a spirit flame to a dull red heat, whilst the current was passing through it, and on cooling I found a similar but stronger permanent torsion of 250°; both currents, as in the previous experiments, having a right-handed spiral. Thus a current of electricity passing through a wire nearly red hot determines a molecular arrangement, or path, which on cooling forces currents of either direction to follow the path which had been determined under the influence of heat.

3. *Molecular Sounds.*

The passage of an intermittent current though iron or other wire gives rise to sounds of a very peculiar and characteristic nature. Page, in 1837, first noticed these sounds on the magnetisation of wires in a coil. De la Rive published a chapter in his "Treatise on Electricity" (1853) on this subject, and he proved that not only were sounds produced by the magnetisation of an iron wire in an inducing coil, but that sounds were equally obtained by the passage direct of the current through the wire. Gassiot, 1844, and Du Moncel, 1878–81, have both maintained the molecular character of these sounds. Reis made use of them in his, the first electric telephone invented, and these sounds, since the apparition of Bell's telephone, have been often brought forward as embodying a new form of telephone. These sounds, however, for a feeble source of electricity, are far too weak for any applied purposes, but they are most useful and interesting where we wish to observe the molecular action which takes place in a conducting wire. I have thus made use of these sounds as an independent method of research, and by their means verify any point left doubtful by other methods, some of which I have already described.

The apparatus was the same as that described in the last section, except that no telephone was used. By means of a switch key the intermittent electric current was either connected with the coil inducing longitudinal magnetism in the wire, or could be thrown instantly through the wire itself, thus rapid observations could be made of any difference of tone or force by these two methods; a reversing key also allowed, when desired, a constant current of either polarity to pass through the wire under observation.

Of all metals that I have yet tried, iron gave by far the loudest tones, though by means of the microphone I have been able to hear them in all metals; but iron requires no microphone to make its sounds audible, for I demonstrated at the reading of my paper, March 31st, that these sounds with two bichromate cells were clearly audible at a distance. A fine soft iron wire (No. 28) is best for loud sounds to be obtained by the direct passage of the current, but large wires (1 millim.) are required for equally loud tones from the inducing coil. By choosing any suitable wire between these sizes we can obtain equal sounds from the longitudinal magnetism or direct current. The wire requires to be well annealed, in fact, as in all preceding experiments, the sounds are fully doubled by heating the wire to nearly red heat. There are many interesting questions that these molecular sounds can aid in resolving, but as I wish to confine the experiments to the subject of the two preceding chapters, I will relate only a few which I believe bear on the subject.

On sending an intermittent electric current through a fine soft iron wire we hear a peculiar musical ring, the cadence of which is due to that of the rheotome, but whose musical note or pitch is independent both of the diameter of the wire and the note which would be given by a mechanical vibration of the wire itself. I have not yet found what relation the note bears to the diameter of the wire; in fact, I believe it has none, as the greatest variation in different sizes and different conditions has never exceeded one octave, all these tones being in our ordinary treble clef, or near 870 single vibrations per second, whilst the mechanical vibrations due to its length, diameter, and strain vary many octaves.

I believe the pitch of the tone depends entirely upon molecular strain, and I found a remarkable difference between the molecular strain caused by longitudinal magnetism and the transversal or ring magnetism produced by the passage of a current. For, if we pass the current through the coil, inducing magnetism in the wire, and then gradually increase the longitudinal mechanical strain by tightening the wire, the pitch of the note is raised some three or four tones (the note of the mechanical transversal vibrations being raised perhaps several octaves); but if we tighten the wire during the passage of an electric current through it, its pitch falls some two or three notes, and its highest notes are those obtained when the wire is quite loose. A similar but reverse action takes place as regards torsion; for if the wire is magnetised by the coil we obtain an almost complete zero of sound by simply moving the torsion index 45° on either side, and as this was the degree which gave silence in the previous experiments for the same wire, it was no doubt due to the same rotation of its polarised molecules. If we now pass a constant current through the wire whilst the intermittent one is upon the coil, we hear augmented sounds, not in pitch but loudness; and if we give torsion of 45° to one side we have silence, or nearly so, whilst, to the other side, it gives increased tones which become silence by reversing the battery. If, whilst the wire by torsion has been brought to zero, we decrease or increase the mechanical longitudinal strain, then at once the polarised molecules are rotated, giving loud sounds; and we further remark that when the wire is loosened, and we again tighten it, we gradually approach a zero, and on increasing the strain the sounds return; thus we can rotate the molecules by a compound strain of torsion and longitudinal strain.

If we wish to notice the influence of a constant current passing through the wire under the influence of the intermittent current in the coil, we find, if the wire is free from torsion, that, on passing the current, the tones are diminished or increased according to the direction of the current. The tones then have an entirely distinctive character, for whilst preserving the same musical pitch as before, the tones are peculiar, metallic, and clear, similar to those given out when a glass is struck, whilst the tones due to longitudinal magnetism are dull and wanting in metallic timbre. If we now turn the index of torsion upon one side, we have a zero of sound with or without the current; but turning in the opposite direction gives increased tones whilst current is passing through the wire, but zero when not. Here again a peculiarity of timbre can be noticed, as although we have loud tones due only to the action of the current through the wire, the timbre is no longer metallic, but similar to that previously given out by the influence of the coil; evidently then the metallic ring could only be due to the angular polarisation of the molecules, and when these were rotated by torsion the tones were equally changed by its action upon the wire.

I have already shown that a permanent magnet brought near the wire can rotate its polarisation, and it equally can produce sound or silence (while the wire is at its zero of torsion, and a constant current is sent through the wire as in the last experiment): we find that either pole of the natural magnet has equal effect in slightly diminishing the sound by an equal but opposite rotation from the line of its maximum effects; but if the wire is brought nearly to zero by torsion, then on approaching one pole of the natural magnet we produce a complete silence, but the opposite pole at once rotates the molecules in such a manner as to produce the maximum loudness, and on taking away the magnet we have comparatve silence as before.

Heating the wire to nearly red heat by a spirit lamp increases the tones of longitudinal magnetism induced by the coil some 25 per cent., but it effects a much more marked increase in the tones produced by the direct passage of the current, which are increased by more than 100 per cent.; and if we pass the intermittent current through the coil and constant through the wire, we find no direct rotation of the molecules by heat. Although an apparent rotation takes place if, by the required torsion, we first place the wire at its zero, for then on the application of heat faint sounds are heard, which become again almost silent on cooling, this is simply due to the diminution by heat of the effect of the elastic torsion.

Tempered steel gave exceedingly faint tones, requiring the use of the microphone; but on magnetising with a constant current, inducing spiral magnetism, the sounds became audible, some 15° sonometer against 175° for iron; thus the molecular rigidity of steel as observed by previous methods was fully verified.

I have mentioned only a few of the numerous experiments I have made by the three methods described, all of which, however, bear directly upon the molecular arrangement of electric conducting bodies. I have selected a few bearing directly upon the subject I have chosen for this paper.

I have, I believe, demonstrated by actual experiments which are easy to repeat, that—

1. An electric current polarises its conductor, and that its molecular magnetism can be reconverted into an electric current by simple torsion of its wire.
2. That it is by the rotation of its molecular polarity alone that an electric current is generated by torsion.
3. That the path of an electric current through an iron or steel wire is that of a spiral.
4. That the direction of this spiral depends on the polarity of the current, or that of its magnetism.
5. That a natural magnet can be produced, having its molecular arrangement of a spiral form, and consequently reversed electric currents would both have a similar spiral in passing through it.
6. That we can rotate the polarised molecules by torsion or a compound strain of longitudinal and transversal.
7. That the rotation or movements of the molecules give out clear audible sounds.
8. That these sounds can be increased or decreased to zero by means that alone have produced rotation.
9. That by three independent methods the same effects are produced, and that they are not due to a simple change or weakening of polarity,

as when rotation has been incomplete a mere mechanical vibration has at once restored the maximum effect.

10. That heat, magnetism, constant electric currents, mechanical strains and vibrations, have all some effect on the result.

III. "On the Identity of Spectral Lines of Different Elements." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received May 12, 1881.

The question of the identity of spectral lines exhibited by different elements is one of great interest, because it is very improbable that any single molecule should be capable of taking up all the immense variety of vibrations indicated by the complex spectrum of iron or that of titanium, and it might therefore be expected that such substances consist of heterogeneous molecules, and that some molecules of the same kind as occur in these metals should occur in more than one of the supposed elements. Further, the supposed identity of certain lines in the spectra of more than one element has been made by Mr. Lockyer the ground of an argument in support of a theory as to the dissociation of chemical elements into still simpler constituents, and in reference to this he wrote ("Proc. Roy. Soc." vol. 30, p. 31), "the 'basic' lines recorded by Thalén will require special study with a view to determine whether their existence in different spectra can be explained or not on the supposition that they represent the vibrations of forms, which, at an early stage of the planet's history, entered into combination with other forms, differing in proximate origin, to produce different 'elements.'"

Young, on examining with a spectroscope of high dispersion the 70 lines given in Ångström's map as common to two or more substances, has found that 56 are double or treble, 7 more doubtful, and only 7 appear definitely single ("American Journal of Science," vol. xx, 119, p. 353), and he remarks, "The complete investigation of the matter requires that the bright line spectra of the metals in question should be confronted with each other and with the solar spectrum under enormous dispersive power, in order that we may determine which of the components of each double line belongs to one and which to the other element." It is this confronting of the bright line spectra of some of the terrestrial elements which we have attempted, and of which we now give an account. For the dispersion we have used a reflecting grating similar to that used by Young, with 17,296 lines to the inch, and a ruled surface of about 3½ square inches; telescope and collimator, each with an aperture of $1\frac{1}{2}$ inch and focal length 18 inches, the lenses being of quartz, cut perpendicularly to the axis and unachromatised, giving a very good definition with monochromatic light. The chromatic aberration is in this case an advantage, for when the telescope is in focus for lines in the spectrum of any given order, the overlapping parts of spectra of different orders are out of focus, and their brightness consequently more or less enfeebled. We have sometimes used green or blue glasses to enhance this result. The telescope and the collimator were generally fixed at about 45°, the collimator being more nearly normal to the grating than the telescope, and the grating moved to bring in successive parts of the spectra. For the parts of the spectra less refrangible than the Fraunhofer line E the spectrum of the third order was employed, for the more refrangible rays that of the fourth order. The source of light was the electric arc taken in a crucible of magnesia or lime; and for the examination of any supposed coincidence first one metal was introduced into the crucible and the line to be observed placed on the pointer of the eyepiece, the second metal was next introduced, and then in most cases, as detailed below, *two* lines were seen where only one was visible before, and the pointer indicated which of the two belonged to the metal first introduced. In some cases where both metals were already in the crucible, we had to reinforce the spectrum of one of the metals by the introduction of more of that metal, which generally brought out the spectrum of that metal more markedly than the other, and enabled us to distinguish the lines with a high degree of probability. Thus the crucibles of magnesia, or the carbons, always contain sufficient lithium to show the orange line and the calcium line heretofore supposed coincident with it (wave-length 6101·9), but we observed these lines quite distinct and separated by a distance, estimated by the eye in comparison with the distance of neighbouring titanium lines, at about one division of Ångström's scale. On dropping a minute piece of lithium carbonate into the crucible the less refrangible line was seen to expand, and for a short time to be reversed, the other line remaining narrow and quite unaltered. When the lithium had evaporated, and both lines were again narrow, a small piece of Iceland spar was dropped into the crucible, which immediately caused the expansion, and on one occasion the reversal, of the more refrangible line, while now the less refrangible line was unaffected.

In this way we satisfied ourselves that the calcium line is the more refrangible of the two, and is probably represented by the line at wave-length 6101·9 in Ångström's normal solar spectrum, while the lithium line appears to be unrepresented.

In the case of iron, which gives such a multitude of lines, it was *à priori* probable that some lines would be coincident, or nearly so, with lines of other elements; and in fact we find that in five-sixths of the supposed coincidences lines of iron are involved. We have, therefore, chiefly directed our attention to iron lines.

Taking iron and titanium, we find that we can resolve the lines at the wave-lengths following:—

6064·7* is a wide double, the iron line less refrangible than the titanium by something like a division on Ångström's scale. Young did not resolve this line, probably because he was looking for a much closer double line.

5714·09. A very close double, the iron the less refrangible, but not in Thalén's list.

5661·65. A double titanium line, but there appears to be a single iron line between the titanium lines.

5489·05. A titanium line, but we could not see any iron line in the arc at this place, and no such iron line is in Thalén's list.

5486·94. The iron line is more refrangible than the titanium.

5446·07. Rather a close double, the iron line the less refrangible.

5428·96. A wider double, the iron line the less refrangible.

5403·28. The iron line in this is the more refrangible.

5396·19. The iron line is the less refrangible.

5006·72. We have not resolved this line; but it is not given as an iron line in Thalén's list.

4990·48. We have not resolved this line; but it is a very strong titanium line, and the titanium may have overpowered the iron line if they are not coincident.

4690·69. The iron line is given by Thalén as slightly less refrangible than the titanium line, but we did not resolve this line (see Appendix to this paper).

4426·9. Not given as an iron line in Thalén's list.

4307·25. The iron line is given by Thalén as slightly more refrangible that the titanium line. We have not resolved this line, the titanium line being weak.

4293·96. The iron is given by Thalén as slightly less refrangible than the titanium, but we did not resolve this line, or make out that there is any titanium line at this place.

4287·47. This is not given as an iron line in Thalén's list.

4171·77. We have repeatedly looked for this titanium line but have not seen it in the arc.

In the case of calcium it was more difficult to resolve close doubles, because the introduction of a very small quantity of a calcium compound into the arc for the purpose of identification caused such an expansion of the calcium lines as to overpower in many cases their weaker neighbours.

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* The numbers by which the lines are here designated are the wave-lengths of the corresponding Fraunhofer lines taken from the catalogue of oscillation frequencies in the Report of the British Association, 1878, p. 37.

6461·98. Not down as an iron line in Thalén's list. If it be an iron line, we are unable to distinguish it from the calcium line (see Appendix).

5601·84. The iron line in this double is less refrangible than the calcium line.

5597·31. A rather close double in which the iron line is the more refrangible.

5348·75. Clearly double, the iron line less refrangible than the calcium line.

5269·59. A very close double, the iron line less refrangible than the calcium.

5041·32. A close double, but distinctly separable, the iron line the less refrangible. Kirchhoff gives the iron and calcium lines as separate, but the calcium line less refrangible than the iron.

4877·57. We could not resolve this line; though Young has resolved the corresponding solar line.

$\left.\begin{array}{l}4585·36\\4578·37\\4580·93\end{array}\right\}$ There are no iron lines at these places in Thalén's list, nor could we detect any in the arc though the positive pole was of iron.

4407·8 seems to be an iron line, though not in Thalén's list, but the calcium line, if there be one at this place in the arc, is so faint that we could not certainly make it out.

4379·16 also seems to be an iron line not in Thalén's list, but we could find no calcium line in the arc corresponding to it.

4375·46 is not given as a calcium line in Thalén's list, and we could see no calcium line at this place.

4307·25 seems to be a very close double, not distinctly divisible, as both lines are somewhat diffuse, but the iron a little less refrangible if it expands symmetrically, and so given by Kirchhoff and by Thalén.

4301·95. The iron line here is faint, and it is more refrangible than the calcium line.

4298·56. Close but distinctly divisible double, the iron less refrangible than the calcium line.

4271·33 had the appearance of a very close double, as when calcium chloride was introduced into the crucible, the line expanded more on its less refrangible side, as if the iron line were rather more refrangible than the calcium line; but we could not definitely separate the two. The two lines are not given as exactly coincident by Thalén.

The lines 4249·81, 4246·89, 4233, 4143·14, 4131·52 are doubtful calcium lines; at least, we could not detect any such lines when calcium chloride was introduced into the arc, though the other calcium lines in the neighbourhood were well developed.

4097·55 does not seem to be an iron line, and we could detect no such line in the arc when the positive pole was of iron.

Of supposed common lines of nickel and iron:—

5168·48 is a close double, but the iron is less refrangible than the nickel line.

5145·87 we could not resolve, but it is not down as an iron line in Thalén's list.

5142·1 is double, and the iron line the less refrangible, but it was difficult to separate the two lines by reason of their diffuse character.

5136·9 is a very close double, the iron line more refrangible and weaker than the nickel line.

4854·85 we have not been able to find any iron line here in the arc.

Of supposed coincident lines of manganese and iron:—

5340·38 is a close double of manganese and iron, but still definitely divisible, the iron line being the more refrangible of the two.

5254·21 is rather a wide double, with the iron line the more refrangible.

4489·49, also plainly double, has the iron line more refrangible than the manganese line.

4414·77 is a close double, but distinctly separable, the iron line the less refrangible.

4054·48. There is a faint iron line here slightly more refrangible than the manganese line.

The magnesium and iron lines in b_4 are separable, the iron line being less refrangible.

Of chromium lines supposed coincident with iron lines:—

5207·78 is plainly double, the iron line being the less refrangible.

5203·88 not clearly resolved, but the iron line seems the less refrangible.

4654·07. We failed to detect any chromium line at this place, though the chromium line at 4646·6 was bright and strong, as well as a line at 4650·5.

4646·6 we could not resolve, but Thalén gives no line of iron in his list at this place.

4369·27 does not appear to be an iron line; we could detect no such line in the arc, and it is not given in Thalén's list. There is an iron line about one division of Ångström's scale less refrangible.

Of supposed coincident lines of iron and cobalt:—

5265·94 is decidedly double, and the more refrangible of the two lines due to cobalt.

5352·57 we did not resolve with certainty, but cobalt expanded the line more on the less refrangible side.

5681·52. The iron line here is decidedly more refrangible than that of sodium.

We have examined further the supposed coincident lines of cobalt and calcium, and of chromium and calcium, and find that 6121·34 is plainly resolvable, and the cobalt line is the less refrangible of the two.

4289·44 and 4274·63 are both close doubles, with the chromium lines the less refrangible. The latter line is difficult to resolve.

5856·6 is plainly resolvable, the calcium line more refrangible than the nickel line.

5480·29. This is a double line of titanium, and the strontium line is more refrangible than either titanium line. They form a not very close triplet.

5424·8. The barium line lies between the titanium line and the iron line next it (5423·7), and the iron line is a very little more refrangible than the barium line.

The indium line 4101·2 we found very difficult to separate from the hydrogen line (*h*), as the latter had to be taken from the spark in a tube, and is both faint and diffuse, but several observations all led to the conclusion that the indium line is very slightly less refrangible than that of hydrogen.

We have also directly compared the iron line at 5316·07 with the solar spectrum, and found that the iron line corresponds with the less refrangible of the two solar lines at this place, so that the coronal line is in all probability the other line of the pair.

There are still a few cases of supposed coincidences which we have not examined. The results which we have recorded strongly confirm Young's observations, and leave, we think, little doubt that the few as yet unresolved coincidences either will yield to a higher dispersion, or are merely accidental. It would indeed be strange, if amongst all the variety of chemical elements, and the still greater variety of vibrations which some of them are capable of taking up, there were no two which could take up vibrations of the same period. We certainly should have supposed that substances like iron and titanium, with such a large number of lines, must each consist of more than one kind of molecule, and that not single lines, but several lines of each, would be found repeated in the spectra of some other chemical elements. The fact that hardly single coincidences can be established is a strong argument that the materials of iron and titanium, even if they be not homogeneous, are still different from those of other chemical elements. The supposition that the different elements may be resolved into simpler constituents, or into a single one, has long been a favourite speculation with chemists; but however probable this hypothesis may appear, *à priori*, it must be acknowledged that the facts derived from the most powerful method of analytical investigation yet devised give it scant support.

Appendix. May 13, 1881.

The following supposed coincidences have been resolved, in addition to those before mentioned:—

6449·27. This is rather a wide double, the calcium line being more refrangible than that of barium.

6461·98. This is a close double, but the iron is less refrangible than the calcium line.

6438·35. A wide double, the cadmium more refrangible than the calcium line.

6407·38. The iron line is decidedly more refrangible than the strontium line.

4690·69. Not resolved before only from the faintness of the iron line, which is less refrangible than the titanium line.

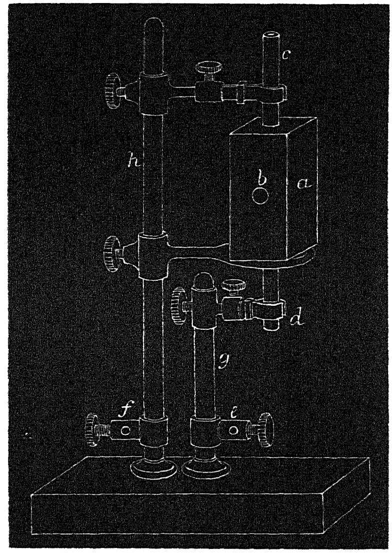

Figure representing the arrangement of crucible by which most of the observations in the foregoing paper were made. *a*, block of dense magnesia; *b*, horizontal hole for observation; *c*, upper perforated carbon through which substances were dropped into the arc; *d*, lower carbon, sometimes drilled and filled with metal; *e*, connexion for positive electrode; *f*, connexion for negative electrode; *g* and *h*, brass pillars fixed on wooden block.

IV. "Observations concerning Transplantation of Bone. Illustrated by a Case of Inter-human Osseous Transplantation; whereby over two-thirds of the Shaft of a Humerus was restored." By W. MACEWEN, M.D. Communicated by Professor HUXLEY, Sec. R.S. Received May 3, 1881.

Facts previously recorded regarding Transplantation of Bone.

Facts relating to attempted osseous transplantations are few, the details concerning them are meagre, and the deductions drawn therefrom have been received with dubiety.

Re-implanted Portions of Bone.—First there are statements to the effect that portions of the skull elevated by the trephine having been replaced shortly after their removal have become again incorporated with and formed integral parts of the osseous walls. Other observers* construe these facts differently. They say that the re-implanted portions of bone have become absorbed, and that they have been replaced by deposits of fresh osseous matter. On the side of the former Flourens adds the weight of his testimony, believing that he has demonstrated† the vitality of the re-implanted portions of bone in the skull of a guinea-pig.

Periosteal Transplantation in Lower Animals.—Ollier has systematically endeavoured to transplant periosteum, and though in one instance he has demonstrated the possibility of successfully transplanting periosteum, and producing permanent bone-growth therefrom, yet most of the grafts either failed at once or having produced certain osseous spicules were then slowly absorbed.‡

Osseous Transplants in Lower Animals.—Osseous transplants were also tried by Ollier, who convinced himself of the success of grafts placed under the skin, and in regions to which ossification was foreign.[*] He judged of the vitality of the osseous grafts by their vascularity, and more especially by the formation of new osseous layers on their periphery. In a rabbit eight months old, he exchanged pieces of the radial diaphysis, transplanting to the left a portion taken from the right, and *vice versâ.* The result was immediate and perfect union in the right radius; in the left suppuration set in and the bone wasted, leaving the periosteum adherent and ossifying.[†] Wolf, of Berlin, repeated Ollier's experiments,[‡] and believed that Ollier had arrived at wrong conclusions, as he (Wolf) had not been able to establish increase of osseous growth in any of his transplants. Ollier, however, maintained his position, and believed it to be strengthened by the fact that rat-tails had been transplanted,[§] and had grown as well after as before transplantation.[||]

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* C'est là du moins la réflexion que fait Wagner sur les faits de Merrem, Wiessmann, Walther, Klencke, Heine, qui lui paraissaient incertains.—"Régénération des Os," par Ollier, vol. i, p. 420.

† Flourens gives the above and other somewhat similar facts in the "Comptes Rendus" de l'Institut, Août 8, 1859.

‡ " . . . Nous avons pu constater au bout de trois ans, sur un lapin, la persistance d'un anneau osseux que nous avions formé autour des muscles profondes de la jambe en transplantant un lambeau de périoste pris sur la jambe du côté opposé."—Ollier, *loc. cit.*, vol. i, p. 413.

"Nous avons vu souvent chez le lapin, le chien, le chat et d'autres animaux, des lambeaux périostiques ou médullaires se durcir d'abord, prendre même une consistence osseuse, et se resorber ensuite peu à peu. En examinant alors le lieu de la transplantation au bout de trois ou quatre mois, on ne trouvait plus rien, ou bien seulement au petit noyau fibreux en voie de disparaître."—Ollier, *loc. cit.*, vol. i, p. 412.

Attempted Transplantation of Bone and Periosteum in Man.—As one might expect, when the materials furnished by experiment on the lower animals are meagre, they are more so when looked for in man. Passing the fabulous account of transplantation of bone[¶] dating from the seventeenth century, the attempt made by Percy to transplant ox bone into man is arrived at, and seems to be the first reliable experiment in that direction.* On two successive occasions he placed in a human lower limb a portion of the tibia of an ox, hoping that it would fill up a gap left by a compound fracture attended with loss of bone. Those attempts were futile. Ollier likewise met with failure in endeavouring to transplant a portion of human periosteum from one part of a man's body to another.† And a like result ensued on an attempt made by another surgeon to introduce a portion of dog's bone into the human body.‡ Some months previous to this last experiment, I transplanted a portion of one of the flat bones of the canine skull into a gap in the skull of a man.§ Two-

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[*] "Pour nous mettre à l'abri des objections qu'on adressait aux expériences de nos prédécesseurs, nous transplantâmes dès 1858 des os entiers sous la peau ou dans d'autres régions étrangères à l'ossification, et nous pûmes nous convaincre que la greffe est bien réelle."—Ollier, *loc. cit.*, vol. i, p. 420; also in "Comptes Rendus" de l'Institut, 28 février, 1859.

[†] Ollier mentions the following experiments:—

"Bagdanowski en 1860 ('Petersburger Medicin. Zeitschr.') a transplanté sur huit chiens des fragments de diaphyse du radius et du cubitus. L'os transplanté ne s'est pas greffé, mais autour de la substance osseuse il s'est formé dans certains cas une capsule ostéo-fibreuse qui se continuait avec les bords de l'os ancien."

"Middeldorpf, ayant introduit des morceaux de radius de pigeon dans la cavité abdominale d'un autre pigeon, vit l'os transplanté subir la dégénérescence graisseuse, après s'être entouré d'une capsule cellulo-fibreuse isolante."—Günsburg's "Zeitschrift für Klin. Med.," 1852.

[‡] "Die Osteoplastik in ihren Beziehungen zur Chirurgie und Physiologie."—"Archiv für klinische Chirurgie," Langenbeck, 1863.

[§] Bert, "De la Greffe Animale," 1863, Thèse de Paris; "Journal de l'Anatomie et de la Physiologie de Robin," 1864; "Comptes Rendus de l'Institut," 1865.

[||] Ollier only succeeded in the rabbit in transplanting portions of bone; he never was able to do so in the dog, as the following will show:—" . . . nous n'avons pas, du reste, réussi à greffer des os entiers chez le chien."—Ollier, *loc. cit.*, vol. i, p. 421.

[¶] "Un ecclésiastique nommé Kraanwinkel, racontait, du temps de Job-à-Méékren, qu'étant en Russie un Seigneur de cette nation reçut d'un Tartare un coup de sabre à la tête, lequel lui enleva une assez grande étendue du cuir chevelu et la portion osseuse correspondante, qui restèrent perdues sur le champ de bataille. Le chirurgien, pour boucher l'ouverture du crâne, détacha de celui d'un chien, tué à cet effet, une pièce d'os de mêmes forme et dimension que celle qui manquait, et l'arrangea si bien que le blessé fut parfaitement guéri . . . . Bientôt les foudres de l'Eglise furent lancées contre lui. Il fallut pour rentrer dans la communion des fidèles qu'il se fît retrancher l'immonde dépouille du chien, quoique solidement consolidée,

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et qu'il se soumît à un traitement plus conforme au caractère de chrétien."—" Dictionnaire des Sciences Médicales," livre XII, p. 355.

* Nous avons deux fois fait cet essai avec de bouts d'os d'avant-bras, pris sur un bœuf, au moment où il venait d'être abattu. Ces bouts avaient été sciés avec soin ; ils étaient encore recouverts d'une partie de leur périoste, et nous les avions interposés entre les fragments de la fracture, arrangés pour les recevoir. Mais notre entreprise, ainsi que nous devions bien nous y attendre, a échoué, et loin que nos pièces d'os de bœuf, après avoir été en place une fois quinze jours et une autre vingt, eussent présenté le moindre vestige d'adhérence et de cicatrization, nous pûmes remarquer, en les retirant, qu'elles avaient manifestement nui au développement vasculaire des surfaces sur lesquelles elles avaient porté et que leur séjour trop prolongé eût fait avorter l'œuvre du cal ou de la consolidation."—" Dictionnaire des Sciences Médicales," livre XII, Art. " Ente Animale," p. 358.

† "13 Mai, 1865.—*Transplantation.*—On fait une incision de 9 centimètres sur la face interne du tibia droit, immédiatement au-dessous du cartilage de conjugaison supérieur, s'éloignant ainsi suffissamment du genou pour ne pas y déterminer une arthrite et se rapprochant le plus possible du cartilage de conjugaison, afin d'avoir la portion de perioste la plus active du tibia. On détache ensuite avec la sonde-rugine un morceau de périoste long de 8 centimètres, large de 2 centimètres, et on le laisse adhérent par un seul point à sa partie supérieure. Faisant ensuite deux incisions transversales de 1 centimètre et demi à 2 centimètres, l'une à la racine du nez, l'autre à 7 centimètres au-dessus sur la partie moyenne du front, on creuse, avec une pince dont les deux branches sont tennues rapprochées l'une de l'autre, une gaîgne dans le tissue cellulaire sous-cutané. On sépare alors le morceau de périoste de ses dernières attaches, et on le transporte dans cette gaîgne . . . . 22 Mai.—Le lambeau paraît avoir descendu ; son extrémité fait plus de saillie par l'ouverture inférieure ; on croit inutile de laisser séjourner un lambeau qui paraît mortifié et qui joue le rôle de corps étranger. On le saisit avec des pinces et on l'arrache brusquement ; le malade pousse un cri, et l'on s'aperçoit que le lambeau tenait encore par trois points qui avaient contracté des adhérences vasculaires."—Ollier, *loc. cit.*, vol. II, pp. 437-8-9.

‡ In September, 1874, Dr. Paterson removed a complete cylinder of the radius of a dog, measuring three-quarters of an inch, and placed it into a gap existing in the radius of a man. The transplant was not successful, the osseous cylinder coming away sometime after in an eroded condition.—" Lancet," October 19, 1878.

§ In March, 1874, while acting as surgeon to the Western Infirmary Dispensary, a man presented himself with a gap in the vault of his cranium, resulting from a wound received some weeks previously. From this wound a detached portion of the thirds of the graft remained in the tissues, apparently becoming incorporated with them and forming a firm hard layer. No opportunity was afforded of demonstrating by an incision the exact condition of the graft. Bearing in mind the repeated statement of Ollier, that osseous grafts taken from one species and transplanted into another species would not live, I am inclined to reserve expressing a positive opinion on the success of the graft in this instance.

Historical Resumé.—The position of transplantation of bone is, therefore, as follows:—In the lower animals Ollier has transplanted periosteum and osseous tissue, and among many failures he succeeded in one instance in forming an osseous ring from grafted periosteum, and in another in transplanting a portion of bone from the one radius to the other of the same animal. Wolf, of Berlin, repeated Ollier's experiments, and stated that he never obtained an osseous growth from any transplant. In man there have been several attempts to transplant periosteum* and bone, which, with one exception, have been attended with failure. The exception—the case mentioned of grafting dog's bone into a human skull—has not been clearly demonstrated.

Points to be solved regarding Transplantation of Bone.

It may be concluded, therefore, that the subject of transplantation of bone and its subsequent growth have not yet been convincingly proved, and that Ollier's experiments require further confirmation.

The questions which are still at issue are—first, will bone grow after transplantation? Will it add any osseous increment to its bulk?

Second, are the facts derivable from animals applicable to man?

internal table of the skull protruded, which, when removed by the fingers, exposed the pulsating brain destitute of its dura mater, but covered by the ordinary exuberant granulations. A somewhat irregular aperture, an inch by half an inch, was left in the osseous wall. In order to fill up this gap a dog six weeks old was taken, and a portion of its parietal containing its ossifying centre was removed along with its periosteum. It was shaped into the form of the gap, and inserted into the osseous aperture in the man's skull. A small hole was left for drainage from the interior of the bone. Before transplanting, the skin which had been adhering to the edges of the bone was elevated, and the bones were refreshed. After transplantation the skin and tissues were drawn together over the graft, and united by sutures. Three weeks after, about a third of the graft was found to have become necrosed, and shortly after this part separated. The remaining two-thirds adhered firmly. Soon after the removal of the necrosed part the wound completely closed. On pressing the finger down on the skull, there seemed to be a little osseous elevation at the part where the graft remained; and as far as the finger could detect through the scalp, the gap was filled by a hard resisting layer, apparently bone.

* In a letter just received from Professor Ollier, he states that he has endeavoured to transplant periosteum removed from the limbs of condemned criminals, placing it in granulating wounds and ulcers. In one instance he found a hard part of cartilaginous consistence, but the after result he was not able to follow.

It is highly necessary to ask this question, as Ollier admits that though successful in his osseous transplants among rabbits, he did not succeed in transplanting portions of bones in dogs.

If the first two questions be answered affirmatively, the third follows:—though the possibility of bone showing indications of growth after transplantation be admitted as a physiological fact, can it furnish any practical result?

Facts tending to show the probability of Transplanted Bone Living.

Facts presented by my surgical observations, especially of compound fractures treated antiseptically, go far to support the belief that transplanted bone is capable of living and growing. Stated briefly they are as follows:—

Portions of completely detached bone covered with periosteum, after being washed with carbolised solutions, have been replaced, and they have lived, thrown out callus, and increased in thickness, uniting at the same time firmly to the neighbouring bone.

A portion of bone, destitute of periosteum and completely detached from the soft tissues, has been removed and placed at a short distance from its original site, where it has adhered, united with the neighbouring bone, and apparently grown thicker in bulk.

The cylindrical extremity of a fractured humerus for over 2 inches was completely stripped of periosteum, round its whole circumference; the soft tissues which covered it in were reduced to pulp, yet this portion of bone, entirely destitute of periosteum, lived and united firmly with the shaft.

Portions of fingers and toes, containing their bones, have reunited after having been completely detached, and have grown into useful members.

The three phalanges of a finger were entirely stripped of the soft tissues and vascular supply; the bones with their ligament and tendons projected as if in an anatomical specimen. The soft parts were put over it like a glove, and both lived and grew.

These were at least sufficient to cause one to look hopefully on transplantation of bones.

Consideration of best Method of Transplantation of Bone.

What elements of Bone are best Suited for Osseous Transplants?—In considering the suitability of the various osseous elements for transplantation, it was evident that periosteum was best suited as far as its vascularity was concerned; but if it were alone taken, it was equally clear that at best only a very thin osseous layer could result, a layer so thin as to be totally inadequate for the formation of a bone resembling the human humerus. It is admitted that the true bone-forming elements of the periosteum are confined to the layer which is in immediate contact with the bone, and which sends prolongations into the Haversian canals. Now, if one attempts to elevate periosteum from healthy bone, portions of the osteogenic layer are apt to be left adhering to the osseous surface, the capabilities for bone formation of the periosteal graft being thereby deteriorated or destroyed.

The whole of the Osseous Elements ought to be included in Transplants of Bone.—In taking the calcareous matter along with the periosteum, the whole periosteal osteogenic layer is preserved, the calcareous matter at the same time forming a support to and mould for the new bone. Besides, the prolongations of periosteum contained in the Haversian canals will proliferate if they have a free surface to expand into, and therefore the soft parts inclosed in the calcareous matter are important elements in the graft. The medulla is also under certain circumstances capable of producing bone; and, through its intermediary, the vascularity of the graft might the more easily be effected. It was, therefore, determined that a graft should be composed of the whole of the osseous elements, periosteum, calcareous matter, and medulla.

Failure of Osseous Transplants due to want of Nutrition.—In transplanting bone the paucity of its vascular elements made the re-establishment of its blood supply the first point to be considered. Failure has attended attempts at grafting cylindrical portions of the long bones, owing to the length of time necessary to establish the blood supply through the periosteum to the more distant and dense portions of bone.

How to Maintain Nutrition after Transplantation.—In order to place transplanted bone in the most favourable conditions for living, it ought to be divided into small pieces, so that the blood effused from the intermuscular space into which the bone would be implanted, may permeate between the individual fragments, and thereby afford a medium for the establishment of vascular connexions, and the supply of nutriment to the individual pieces of bone. Though the transplant consisted of dense osseous tissue, in its divided form it would somewhat resemble cancellated tissue. The re-establishment of the blood supply to the grafts would thus be hastened and facilitated, and any portion of the graft which was not suitable could be easily thrown off without involving the whole.

Other reasons for Dividing Bone into Small Pieces.—On other grounds the division of the osseous transplant into small portions is advantageous. Whether the leucocytes in the effused blood clot may or may not be transformed into bony tissue, the blood clot itself when small in quantity forms an excellent matrix for the proliferation of the osteogenic elements. Again, the division of the bone into small pieces not only makes the individual vitality more certain, but it also supplies a greater number of proliferating osseous centres. In the case of the calcareous tissue there is this further potent reason, that the osteogenic cells contained in the Haversian canals cannot proliferate (except at the expense of the hard tissue) as long as they are bound in—imprisoned by—calcareous walls; the division of these hard walls gives the soft tissues contained therein room to proliferate. Besides, the graft when composed of small portions of bone, is more easily moulded into any form which the surgeon may desire.

It was resolved to give practical expression to these views in the following case. In order to place all the particulars of the case before those interested, the history is given in detail.

Case in which Interhuman Transplantation was Successfully Performed.

William Connell, aged three years, was admitted into the Royal Infirmary, Glasgow, under my care on the 17th July, 1878, in a much emaciated and exhausted condition arising from suppuration in connexion with necrosis of the right humerus.

He came of healthy, though poor and itinerant parents. He presented the appearance of a much neglected child. Constitutionally he was weak, his pulse a mere thread, his cheek surmounted by a hectic flush, and on slight effort beads of perspiration bedewed his forehead. The right upper arm was greatly distended, and fluctuant from shoulder to elbow. About the middle of the upper arm there were two cicatrices, which were stated to be the marks of recent openings through which pus had escaped to within a week of his admission to the hospital. Toward the middle of the outer aspect of the arm, the skin, including these cicatrices, was for about a couple of inches thin and reddened. A puncture was made at this point, giving vent to fourteen ounces of thin fœtid pus. When the abscess was evacuated the shaft of the humerus was found to be totally necrosed, and already separated from its head at the epiphyseal junction. At the condylar epiphyses slight crepitation was elicited. From the condition of the bone, as well as from the few items of history which could be relied on, it was evident that the destruction of the bone had occurred from four to six weeks previously. After the pus had been evacuated, and the distention of the soft parts had thereby been reduced, the shaft of the humerus was exposed through an aperture measuring nearly an inch square, situated on the outer aspect of the arm. The bone at this point was dark-coloured and fœtid. The arm was dressed, the patient was placed on generous diet, and otherwise attended to in the hope that his strength might improve, and that he would thereby be placed in a better condition for the removal of the necrosed shaft. Notwithstanding treatment, the daily discharge of fœtid pus was great, and as the amount was not much lessened at the end of three weeks, it was considered advisable to remove the source of irritation.

Necrosed Bone Removed.—On August 7th, 1878, the part of the necrosed bone exposed at the aperture already mentioned was divided by the bone forceps. The upper part of the shaft was then caught by lion forceps, rotated in order to loosen its periosteal attachments, and then pulled out. The lower portion was similarly dealt with. The two portions removed comprised the whole humeral diaphysis. The periosteum opposite the opening in the soft parts was nowhere seen or felt, its place being occupied by granulation tissue. Granulations likewise lined the tunnel from which the bone was removed; but at the upper part toward the head of the bone, the finger detected periosteum with rough calcareous matter adhering. The apertures left by the removal of the shaft were stuffed with carbolised lint, and the arm was carefully fixed on a splint. The suppuration continued profuse for about a fortnight, then gradually diminished. The tunnels left by the withdrawal of the bone were kept patent as long as possible, but they slowly coalesced from the epiphyses toward the superficial openings without the formation of new bone, except for a short distance from the head of the humerus. After the first fortnight the patient's constitutional state improved under good diet, cod liver oil and lime water. Local applications, with a view of stimulating the bone growth, were tried, but were found unavailing. The wounds were quite healed on 1st November, and he was dismissed from the ward on the 23rd November, 1878, with about an inch and three-quarters of shaft attached to the head of the bone; even this was thin and tapered. There was no bone from this down to the condyles. There was, therefore, over two-thirds of the humeral shaft wanting. A month after (in December, 1878), he was seen, and the tapered portion had increased by about a quarter of an inch. The measurement from the acromion process to the end of this tapered process was 2 inches. He was seen monthly for some time, but no further growth of bone followed.*

Appearance of the Arm one year and three months after.—On November 7, 1879, the patient was re-admitted to the hospital. The bone had not grown further, though one year and three months had elapsed since the subperiosteal resection of the humerus. When the limb was placed hanging down by the side, the measurement from the tip of the acromion process to the distal extremity of the proximal portion of the humeral shaft was nearly 2 inches. In form, this upper fragment was conical, tapering from the head to a narrow spike-like distal extremity, which appeared, when he attempted to raise the arm, as if it would penetrate the skin. From this down to the condyles there was a complete absence of bone, there being nothing but soft tissues in the gap. The muscular power was good, but when he attempted to raise his arm a contraction of the muscles took place, the condyles being drawn towards the proximal extremity, while some fibres of the deltoid raised the spike-like process of the upper portion, causing it to project as if about to penetrate the skin. Here the action ceased, the soft parts in the gap appearing like a rope during the contraction. He could not raise his forearm to his breast. If one caught the arm firmly with the hand so as to keep the condyles fixed and separate from the upper fragment, then the patient could elevate the forearm towards his chin. The power was there; the lever and fulcrum were wanting. An apparatus supplying these might have been devised, but, if such an expensive article could have been obtained, it would have been necessary to renew it often as he grew older. On account of his social condition it would have been impossible to secure this and the after attention necessary. The only other alternative was to supply the gap by transplantation of bone.

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* Cases of subperiosteal resections of the humerus have occurred in which the bone has not been reproduced. Thus, Nedöpil performed a subperiosteal resection of the humerus in a boy twelve years of age, the shaft not being reproduced. In order to render this boy's arm of some use an orthopœdic apparatus, devised by Billroth, was applied. Neudörfer also mentions a case in which the humeral shaft was not reproduced after subperiosteal resection. (Professor D. Vogt, "Die Chirurgischen Krankheiten der Oberen Extremitäten," p. 225, paragraph 212.)

In my wards there were numerous cases of marked anterior tibial curves, from which wedges of bone had to be removed, and it was determined to utilise these wedges as transplants.

Transplantations of Bones.

Transplantation of two Wedges of Bone.—On November 9th, 1879 (one year and three months after the removal of the necrosed shaft) an incision was made down to the extremity of the upper fragment. This extremity was found to be cartilaginous for fully a quarter of an inch. This cartilaginous spike-like process was removed, leaving then a portion of bone which measured 1¾ inch from the tip of the acromion process. From this point a sulcus about 2 inches in length was made in a downward direction between the muscles. The former presence of bone was nowhere indicated, and the sole guide as to the correct position into which the transplant was to be placed was an anatomical one. After the sulcus was formed, the hæmorrhage was fully arrested, and an aseptic sponge was placed in the gap, which was then ready to receive the bone.

Two wedges were then removed from the tibiæ of a patient six years of age, affected with anterior tibial curves. The base of these osseous wedges consisted of the anterior portion of the tibia, along with its periosteum, the wedges gradually tapering towards the posterior part of the tibæ.

They were removed, then cut into small fragments with the chisel, and immediately thereafter they were deposited in the sulcus in the boy's arm. They were kept under the spray from the time they were removed from the tibiæ until they were covered by the soft parts of the arm and its antiseptic dressing. The time occupied in removing the wedges, cutting them into fragments, and placing them in the living tissues of the arm, was about two to three minutes. A drain of horsehair was inserted, and the wound was carefully stitched and dressed.

The wound healed without pus production. The patient's temperature remained normal throughout.

A month after the operation the arm was looked at for the third time, when the transplant felt firm and united.

Result of the first Transplant.—Two months after, it was thoroughly examined, when a portion of the bone, 1 inch in length and, as far as could be measured, nearly three-quarters of an inch in thickness, was found firmly attached to the upper fragment of the shaft. In running the finger from the head of the bone towards the graft, the latter could be distinguished by its greater breadth. At this time, instead of the former sharp spike, the upper fragment ended in an obtuse terminal.

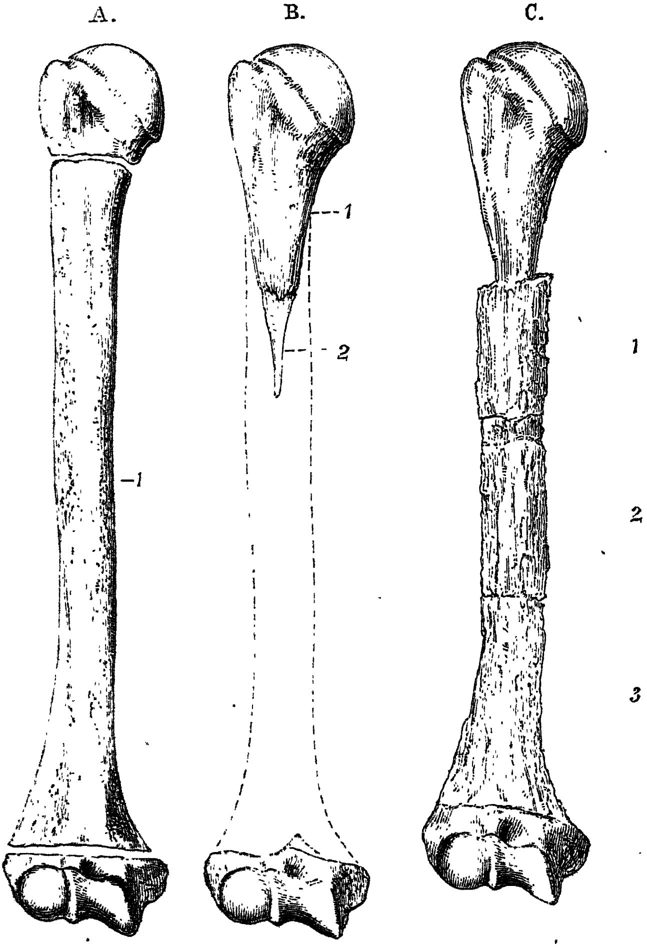
Placing the arm by the side, the measurement from the tip of the acromion process to the extremity of the bone was now 2¾ inches. That was a distinct gain of 1 inch in length. The arm was shown in this condition to the Glasgow Pathological Society.

Second Transplant.—On February 1st, 1880, the upper fragment was laid bare, the extremity of the first graft was exposed, and found to be covered by a fibrous vascular membrane, which had to be elevated in the same way as periosteum. Under this there was distinct calcareous tissue, which had all the appearance of living bone. A sulcus was made between the muscles for the reception of fresh grafts, which were obtained from a patient five years of age, affected with anterior tibial curves. The wedges were divided into larger pieces than the first graft.

Some of these measured half an inch by a quarter of an inch, but the majority were about one-eighth of an inch by one-sixteenth.

The child kept well up to the third week, when the temperature suddenly rose, and on examination some pus was found on the dressing, and one of the larger pieces of bone was discovered on the surface of the wound. Afterwards several portions of bone were shed. The portions were mostly the larger pieces of the graft. The smaller fragments remained. Thus about one-third of the second graft came away. The portions of bone which were shed were eroded, small hollows being formed on their surface, filled with granulation tissue. After these parts were removed, there was a distinct firm mass of bone left.

Result of the second Transplant.—On April 20th (about two and a half months after the second graft) the measurement from the tip of the



Schematized Drawings.

A.

1. Necrosed diaphysis, which was removed.

B.

1. Portion of shaft attached to head reproduced from original periosteum.
2. Cartilaginous terminal removed before first transplant.

C.

1. First graft.
2. Second graft.
3. Third graft.

acromion to the extremity of the shaft was 4 inches, being a gain of an inch and a quarter.*

The third Transplant.—On 9th July, 1880, the third transplant was performed. On this occasion the condyles were exposed, and a cartilaginous spike an eighth of an inch in length, situated between the condyles was seen to be the sole representative of the shaft from that direction. This cartilaginous spike was removed, and the upper portion of the condyles was refreshed. A sulcus between the muscles was then made from the condyles to the distal extremity of the upper fragment. Two osseous wedges were removed from a patient aged nine years, affected with anterior tibial curves. Each of those wedges was half an inch thick at its base, and measured three-quarters of an inch in length from apex to base. They were divided into smaller fragments than the last, and were laid into the groove between the muscles. The wound healed, all but a small part of the lower border, from which four small portions of dead bone were removed at the second and third dressings. About the middle of August there remained a firm osseous ridge running from the condyles towards the upper fragment. At this date the patient was dismissed from the hospital, and returned on 27th October, 1880. On his return the last transplant was found to be still firm, and measuring from 1¾ to 2 inches in length. It was also thicker than that resulting from the others. The proximal and distal grafts were now touching and could be rubbed together.

Two Ends of transplanted Bones refreshed.—It was thought advisable to refresh the ends of these grafts, and bring them together by sutures. This was done on October 31st, 1880 (over three and a half months from the last graft, and about one year from the time of the first graft). Both extremities were seen to be distinctly osseous, and appeared to be living bone. They were covered by a fibrous vascular membrane, resembling periosteum. It was somewhat difficult to secure correct apposition, and some portions had to be removed by the bone forceps in order to permit of better adaptation. Six weeks after, when the wound was looked at, union was not perfect. The extremities of the bones were rubbed firmly together, and on January 9, 1881, union was found to be taking place. In order to hasten it a couple of pegs were inserted with the aid of a drill. They were kept in for five weeks, and when removed the bone was found firm. At the beginning of March, 1881, the bone was found firmly united, from the head to the condyles, and measured 6 inches; while the left humerus measured 6½ inches, that is to say, half an inch longer than the transplanted one. On rotating the condyles the head of the humerus responded to the movement. The patient could lift his arm to his head and otherwise use it.

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* On July 6th, 1880, a little girl was admitted into the ward suffering from a crush of the upper arm received by a railway accident. Amputation at the upper third of the arm had to be performed. The lower third of the humerus, though somewhat crushed, was so nearly what was wanted to fill up the gap in the boy's arm, that it was determined to transplant it. Owing to the manner in which it fitted into the gap in the boy's arm it was considered advisable, in an experimental sense, not to divide it into small pieces, but to preserve it entire. It was transferred on July 6th, but three days after the periosteum was found to be in great measure stripped, leaving the cylinder bare. The bone was therefore removed; the granulations in the sulcus between the muscles scraped, and what may be regarded as the third regular transplant proceeded with.

Résumé.—This was a case of extensive suppurative periostitis followed by complete necrosis of the humeral diaphysis. The necrosed humerus was divided at its exposed part, and each half was pulled out from what was supposed to be its periosteal sheath; but on withdrawal doubts were expressed as to whether the periosteum had not, for the most part, undergone destruction. As a result, at the proximal extremity bone was formed, of a pyriform shape, tapering from the head toward a point an inch and three-quarters from the tip of the acromion process. This left over two-thirds of the shaft wanting. There was no further attempt at bone formation. A year and three months afterwards, the state of the parts remained the same. After this time the first transplant was performed. In making the sulcus for the reception of the graft, reliance had to be placed on the anatomical relations as to the correct position for the graft, as there was no trace of periosteum or fibrous structure to indicate the former whereabouts of the bone. Portions of human bone were transplanted on three different occasions. The grafts were obtained from patients affected with anterior tibial curves, from whom wedges of bone had to be removed for the purpose of straightening their limbs. These osseous wedges, with their periosteum, were each divided, after removal, into many small pieces, which were immediately placed in the sulcus prepared for them in the boy's arm. These small portions united together and adhered to the head of the humerus above and the condyles below, ultimately forming a solid rod, only half an inch shorter than the humerus of the opposite side. The transplantation of bone converted a useless arm into a thoroughly useful one.

Deductions from these Transplantations.

Though the foregoing is only a single case, as far as the individual is concerned, yet from the number of transplants which have been performed it may be regarded as a series of experiments. It is now necessary to ask, what conclusions may be drawn from the data supplied by these experiments? Before answering these, the way may be cleared by reference to two points. First, some who have heard of this case without having seen the operations, have asked, whether it was not possible that some old periosteum remained in the arm and produced the new bone, the operation of transplantation having only acted as a stimulus to it? In answer, it must be borne in mind, that had periosteum existed, between the condyles and the upper part of the humerus, it had ample opportunity of revealing itself by osseous growth, during the fifteen months which elapsed between the removal of the dead bone and the transplantation of the new. Again, in opening up the sulcus between the muscles for the reception of the transplants, neither periosteum nor any vestige of like fibrous membrane was seen; so much so, that it was only by recognising the relative positions which the muscles ought to occupy toward the humerus, that a guide to the correct position of the transplants was found. Further, the growth of bone in the arm was at first only commensurate with the transplants. There was no indication of osseous growth in the vicinity of the transplant which might have arisen from the "supposed stimulated periosteum." Finally, the solid humerus still retains the irregularities of shape which the transplants were permitted to assume in the tissues. So that there is not an iota of fact to support the supposition that the new bone grew from old periosteum.

Transplants not being Absorbed.

Secondly, it is stated that though transplanted bone may be retained in the tissues, yet it may be simply encapsuled and be undergoing slow absorption. If absorption were taking place the changes so brought about ought to be apparent by this time. The first graft was made one year and five months ago, the last nine months since (this is written on 12th April, 1881), and the bone formed after the healing of the wound made for the reception of the graft has not only maintained its original dimensions but has grown. This sufficiently sets at rest the question of absorption of the transplanted bone.

The way being thus clear, the conclusions drawn from the data supplied by these experiments may be considered. To commence with the questions in the order in which they are placed at the beginning of this paper. What evidence do these experiments afford as to the growth of transplanted bone?

Proofs of Growth of Transplants.

When from six human lower limbs six wedges of bone (along with their periosteum and marrow) are removed, divided into small pieces, placed in the arm of a boy, in an intermuscular space freshly opened by the scalpel for their reception; and when it is seen that, with the exception of a few fragments of bone which were shed, the whole of the grafted portions united one to another and to the ends of the original bone; making in all four and a-half inches of osseous transplant, whereby a united humerus has been formed, which he moves and uses as the other arm; it may be concluded that the transplanted bones have lived and grown. Before these transplanted portions of bone could have united together, there must have been a proliferation of the bone-forming elements contained in the grafts. That the development of fresh bone originated in the transplants is typically illustrated, where the second transplant united to the first by one end, while its other extremity was free in the tissues; so that it did not come into contact with the original bone at any part. Yet this transplant consolidated just as well as the first or second transplant did. As further illustrations of the vitality of the transplants and the growth of new bone from them, there is, firstly, the evidence derivable from the callus thrown out between the second and third transplants, after their extremities were refreshed, the same phenomena being observed here as in an ordinary case of ununited fracture. Secondly, the bones became sensibly thicker at the point where they were drilled for the reception of the pegs. All these are evidences of actual growth of bone from the transplants.

Vascularity of Transplants.

When the extremities of the second and third transplants were refreshed, the appearance of both bones was that of living osseous tissue; surrounded by a thin fibrous vascular membrane closely adherent to the bone, and which bled when it was scraped up, much in the same way as periosteum would under similar circumstances. This membrane did not resemble the thick semi-vascular capsule which is found surrounding dead tissue in process of being absorbed.

These facts being derived from the interhuman transplantation are, as they stand, a sufficient answer to the second question; while the utility of the arm after restoration of two-thirds of the humeral shaft establishes the practical results of the operation.

The method of division of the bone into small pieces prior to transplantation, and the *à priori* reasons for doing so, have been borne out by the success which has attended the practical performance of the operations.

Conclusion.

From the foregoing the following may be formulated:—

1. Transplanted bone is capable of living and growing.
2. Interhuman transplants of bone live and grow.
3. Interhuman osseous transplants are capable of being put to practical uses beneficial to mankind.
4. The whole of the osseous elements ought to be included in the transplants.
5. The most successful mode of transplanting bone is to divide it

into small fragments with a sharp instrument before inserting it into its new locus.

6. In order to insure the success of the graft the transplantation must be conducted antiseptically.

V. "Experimental Determination of the Velocity of White and of Coloured Light." By Dr. J. YOUNG, F.R.S., and Professor G. FORBES. Received May 17, 1881.

(Abstract.)

The method employed in this research to measure the velocity of light resembled the method of M. Fizeau, subsequently employed by M. Cornu. A revolving toothed wheel is employed in the same way to alter the intensity of the light reflected from a distance. In the present method, however, there are two distant reflectors instead of only one. They are separated by a distance of a quarter of a mile. The observing telescope and the two reflectors are almost in the same line. The observer sees two stars of light which go through their phases with different periods as the toothed wheel is revolved at increasing speeds. One star is increasing, while the other is diminishing, in intensity, with increase of speed of the toothed wheel. The speed required to produce equality of the lights is determined by means of a chronograph. By choosing such a speed as gives a maximum of one star at the same speed as a minimum of the other, a pair of observations eliminates all cause of doubt arising from varying brightness in the stars, and ratio of the width of a tooth to the width of a space. The distances were observed by triangulation with the Ordnance Survey 18-inch theodolite, using as a base line a side of one of the Ordnance Survey triangles. The source of light was an electric lamp. The velocities (uncorrected for rate of clock, and reduction to a vacuum) measured are as follows:—

187,707
188,405
187,676
186,457
185,788
186,495
187,003
186,190
186,830
187,266
188,110
188,079

Mean...... 187,167 miles a second.

The correction to vacuum is + 54 miles a second. The correction for rate of clock to a mean solar time is + 52 miles a second.

The final results for the velocity of the light from an electric lamp *in vacuo* is 187,273 miles a second, or 301,382 kilometres a second.

Using Struve's constant of aberration 20″·445, we obtain for the solar parallax the value 8″·77, and for the mean distance of the sun 93,223,000 miles.

On February 11th, 1881, the reflected stars were seen to be coloured, one reddish, the other bluish. The particular colour of a particular star depended upon the speed of rotation of the toothed wheel. That star which was increasing with increase of speed of the toothed wheel was reddish, that one which was diminishing with increase of speed was bluish. This seems to be caused by the fact that blue rays travel quicker than red rays.

A number of tests were made to judge of the accuracy of this conclusion, and they confirmed it. In the final arrangements, the electric light was acted upon by a bisulphide of carbon prism, and part of a pure spectrum was used. Differential measurements were then made to find the difference in velocity of rotation of the toothed wheel, required to produce equality of red and of blue lights. The most convenient method was to use a driving weight slightly in excess of that required to produce equality of the light, then to fix to the pulley carrying the weights one end of a piece of stout india-rubber tubing, the other end being fixed to a point above. This gradually diminished the effective driving weight. The equality of red lights was first noted, the colour of the light was changed, and the interval of time until the blue lights were equal was measured. The rate at which the india-rubber diminished the speed was afterwards measured by the aid of the chronograph, and thus the difference of speed determined. The mean of 37 determinations in this and other ways gave the result that the difference in velocity between red and blue lights is about 1·8 per cent. of the whole velocity, blue travelling most rapidly.

The general conclusion seems to be supported by a comparison of the velocity of light measured by M. Cornu and Mr. Michelson, where the source of light usually employed is taken into consideration. These are the only accurate measurements of the velocity of light hitherto published. They give us the following results :—

	Usual source of light.	Velocity in kilometres a second.
Michelson's research....	The sun near horizon..	299,940
Cornu's "	Lime light...........	300,400
The present "	Electric light	301,382

Classifying the sources of light used by Cornu, we get the following approximate relative velocities:—

Source of light.	No. of observations.	Approximate relative velocity.
Petroleum.................	20	298,776 kilos.
Sun near horizon...........	77	300,242 „
Lime light	449	300,290 „

All these results seem to support the view that the more refrangible the source of light, the greater is the velocity. But the evidence of the present observations, indicating an excess of velocity for blue over red light, seeming to exceed 1 per cent. of the whole, must rest upon the merit of the present observations themselves.

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"On the Absorption Spectra of Cobalt Salts." By WILLIAM J. RUSSELL, Ph.D., F.R.S., Treas. C.S., Lecturer on Chemistry at the Medical School, St. Bartholomew's Hospital. Received August 4. Read November 18, 1880.

The following investigation was commenced at the suggestion, and indeed in conjunction with, Mr. Lockyer. Other work, however, prevented his continuing with me this research, but I am much indebted to him not only for instruction in methods of spectroscopic observation, but also for important suggestions and much aid in the course of the work.

The more immediate object of the investigation was to examine carefully and systematically the absorption spectra produced by certain coloured liquids and solids, in order to identify the spectra with the substances producing them. If this be fully done, a powerful means has been obtained for investigating the changes, be they either chemical or physical, which occur more especially in cases of solution. Of all coloured substances the cobalt salts seemed most suitable for commencing the investigation with, as they give well marked band spectra, and are salts well known and easily obtained.

The following observations were made with a Dasaga's spectroscope having a single heavy glass prism, and, as a rule, it did not seem advisable to use higher dispersive power. The positions of the bands are given in wave-lengths of millionths of a metre, obtained from the observations by graphical interpolation.

Cobalt chloride was the salt first examined; the absorption spectra produced by it when pure and dry were carefully ascertained, and then the changes occurring in the spectrum when it was acted on by water and by other substances. First, with regard to the pure cobalt chloride ($CoCl_2$). A specimen of the pure salt was fused out of contact with the air in a glass tube. Another specimen of this salt was obtained by dissolving pure cobalt in hydrochloric acid, fusing it in an atmosphere of that gas, and sealing it up in a glass tube to exclude moisture. Fig. 1 represents the absorption spectrum which was obtained in both cases. When in place of pure the commercial salt was used, when the salt was formed by the action of chlorine on the metal, when the ordinary hydrated cobalt chloride was dried and fused in hydrochloric acid gas, when in place of hydrochloric acid, an atmosphere of carbon dioxide was used; in all these cases the absorption spectrum remained precisely the same: these experiments then show that the above is the visible spectrum belonging to the true cobalt chloride when in a fused state. The chloride fuses at so high a temperature that the glass becomes red hot, and it has consequently not been found possible to observe the absorption spectrum of the chloride when in a liquid state.

One point further: this spectrum is not in any way due to the action of the fused chloride on the glass tube, for water entirely dissolves the salt, leaving the glass colourless.

Whether in fig. 1 both bands are due to the same substance, and whether the small absorption at the red end of the spectrum is accidental or not, cannot be discussed at present.

If bromide of cobalt be treated in a like manner to the chloride, it yields a very interesting spectrum (shown in fig. 2), for on comparing it with the chloride spectrum a strong similarity between them is at once seen; but the bands of the bromide corresponding to those of the chloride have approached the red end of the spectrum, the most refrangible band more than the other one. Owing to the opacity and the ease with which the iodide of cobalt decomposes, its absorption spectrum cannot be obtained in the same way.

The strong power which the cobalt chloride has of combining with water, and the change of colour and of spectrum which it undergoes, is well known. The alteration thus brought about in the absorption spectrum will be discussed further on; but it should here be stated that simply on drying the moist chloride without fusing it, a spectrum is obtainable similar to that from the fused salt. The dehydration of the ordinary crystallised chloride can be effected even at a comparatively low temperature; if the heat be continued for a considerable length of time, a temperature of 150° if continued for 4½ hours renders the salt dry. It is not easy to see the absorption spectrum of the powder thus formed; the plan found to answer best is to have the salt in a fine state of division, suspend it in melted paraffin, and then when cold cut slices of the paraffin of proper thickness.

Assuming, then, that the above is the true spectrum of the solid cobalt chloride, the next point was to study spectroscopically the changes it underwent when acted on by different substances, or even when merely its physical state was altered.

The action of water on the chloride is complicated, and it is better to consider first the simpler case of the chloride fused with potassic chloride. When these two substances are mixed, especially if mixed in atomic proportions, the whole mass fuses at a considerably lower temperature than either salt alone; it forms a greenish-blue mass, and even when fused for some length of time in air it undergoes no

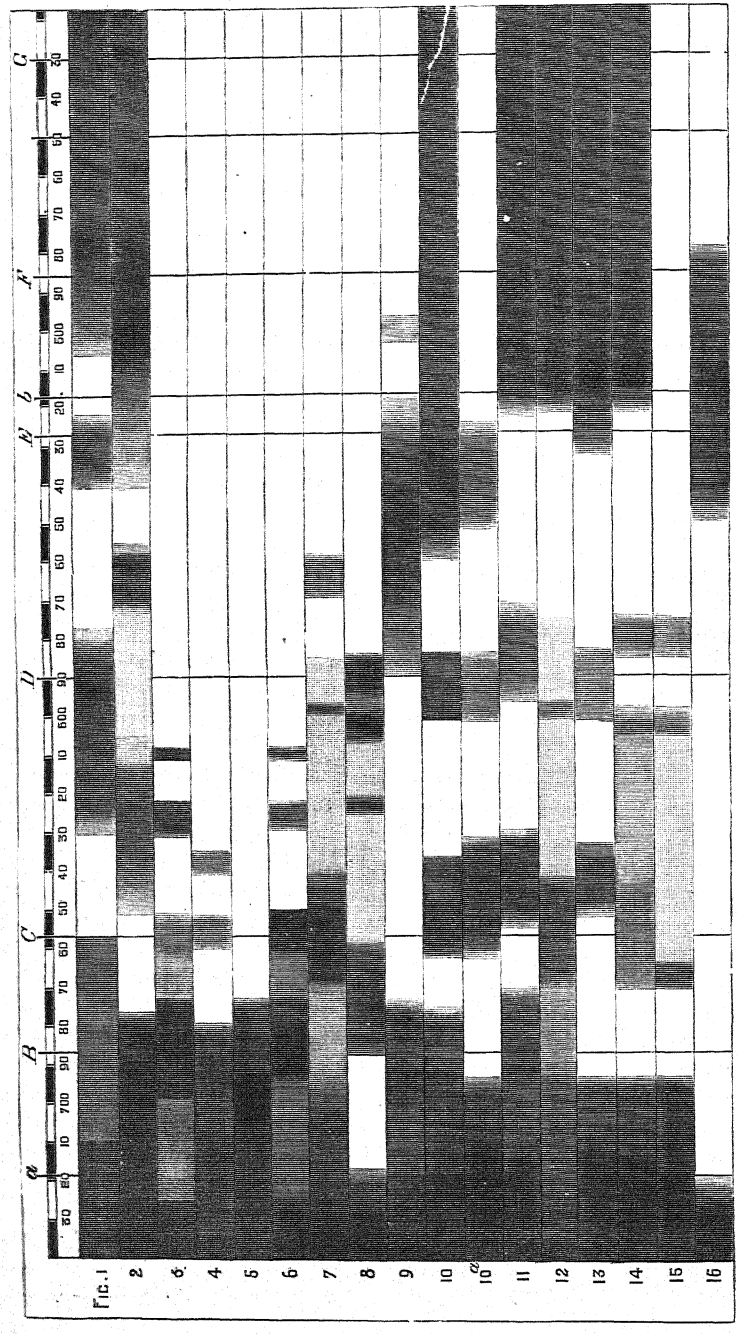
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Fig. 1

decomposition. On examining this fused mixture it is found to give an absorption spectrum (shown at fig. 3) quite different from that given by the chloride alone. On varying the proportions of the two salts, even taking as much as twenty molecules of the potassium salt to one of the cobalt salt, still the spectrum remains the same; in fact, if only a trace of the fused or hydrated cobalt chloride be added to potassium chloride, the fused mass gives this same spectrum. On increasing the thickness of the layer of salt examined or the amount of cobalt present in a layer of constant thickness, the absorption extends between the bands, and quickly reaches 600. Now, when the absorption has reached thus far, a faint band from 540 to 535 becomes visible, a band agreeing in position and character with the one produced by the pure chloride; increase the absorption still more, both sides close in, leaving only a band of light from 580 to 550. On making a similar experiment with the bromide of cobalt, fusing it with potassium bromide, an absorption spectrum is obtained (given at fig. 4), and if the iodide of cobalt and potassium iodide be fused together, a spectrum (shown at fig. 5) is obtained; the first two spectra are strikingly alike. In the above order the absorption bands are nearer and nearer to the red end of the spectrum, and only one band of the iodide is clearly visible, still one cannot but suspect the existence of the others, although not visible. With a considerable thickness of the bromide a band from 573 to 558 is very visible, the analogy of the 532 band in the chloride. Fluoride of cobalt (for a specimen of which I have to thank Dr. Gore), neither alone nor with potassium fluoride, gave any visible absorption bands.

In place of potassium chloride, sodium chloride, ammonium chloride, zinc chloride were used; they all, when cold, gave the same absorption spectrum, but with this peculiarity, that the exact position of the two bands between about 600 and 625 varied with each salt to a small extent. With the potassium chloride the bands were nearest to the red, and with the zinc chloride nearest to the blue.

The distance between these two bands appears to remain nearly if not quite constant. With regard to the absorption which extends from 650 to 700, if only a thin layer of the salt be used, it will be clearly seen that it is resolvable into a dark band from 696 to 675, and a fainter band at 656 (figs. 3 and 6). The action of heat on these bodies is curious and interesting, the absorption at the red end of the spectrum is intensified, the bands disappear in a mass of dark absorption, and with regard to the two bands in the orange they gradually merge into a single band; and on still further increasing the heat (but at a temperature still considerably below the fusing point of the mixture) the band at 625 disappears, and there is a broad band beginning sharply about 620, and extending nearly up to 600. On cooling, precisely the same changes are gone through in reverse order. On repeating the experiments over and over again, and continuing the heating for a long time, there is always the same result, the return on cooling to the original spectrum.

From the change in the fusing point of the mixture, from the permanence of the cobalt chloride when the mixture is fused in the air, and from the change of colour, it seems natural to conclude that chemical combination takes place between the chlorides, and that the new spectrum pertains to the double chloride then formed. Experiments, to be described further on, will show, however, that this supposition is not correct; for this spectrum (fig. 3) is easily obtainable without the presence of any alkaline chloride, and that it is produced, in fact, in all cases when the cobalt chloride dissolves in any menstruum, without chemically combining with it, to form a new and definite compound. At ordinary temperatures then, at all events, the two chlorides in the above mixture are not chemically combined, but the cobalt salt is simply dissolved in the potassium or other chloride.

If these mixtures be dissolved in water, on evaporation the two salts separate out perfectly distinct, the potassium chloride often even without colour. Zinc chloride gives a different reaction from the alkaline chlorides, for when mixed in certain proportions this spectrum is no longer visible. On evaporating an aqueous solution of these two salts, a new compound, a double salt having the composition $CoCl_2ZnCl_2+6H_2O$, crystallises out. This double salt produces no visible banded spectrum, consequently as long as sufficient zinc chloride is present to combine with the cobalt chloride, no spectrum is visible; but when even a trace of cobalt beyond this amount is present the banded spectrum (fig. 3) is visible.

Some interesting applications of these changes have been made and will form a separate communication.

The visible spectroscopic changes which occur when these mixed chlorides are heated, are as above described, viz., a new spectrum is produced, as if chemical combination occurred, and on cooling the return of original spectrum. This seems to indicate that the action of heat is to produce change of a physical rather than of a chemical nature; that a dissociation of a cobalt salt into a simpler molecular form occurred rather than the formation of a new chemical compound, which always decomposed as the temperature fell. With zinc chloride, however, the action of heat is different and easily explainable, for a mixture of cobalt and zinc chlorides, which at ordinary temperature gives no bands, on being heated shows very clearly this same band spectrum of cobalt chloride. In this case heat breaks up the double salts existing at ordinary temperatures, and gives the spectrum of the free chloride. If calcium chloride be used in place of potassium chloride, then a much higher temperature is required to fuse the mixture, and it is far more opaque and difficult to examine. There is, however, this point of interest about it, that it gives the spectrum of the simple fused chloride; this may arise either from some dry chloride having escaped the action of the calcium chloride, or what seems more probable, that it arises from the action of the high temperature on the cobalt.

There are a few liquids which appear to dissolve the anhydrous cobalt chloride without combining with it, and it was of much interest to see what spectra they would give. The liquids experimented with were hydrochloric acid, methylic, ethylic, and amylic alcohols, the saline ethers, amylene, glycerine, and glacial acetic acid.

First, with regard to the action of the ordinary strong hydrochloric acid. As is well known, cobalt chloride dissolves readily in this acid, yielding an intensely blue solution. The spectrum which this liquid gives is shown at fig. 6, and it will at once be seen to be similar to the spectrum obtained from the fused chloride (fig. 3), the two bands in the orange now being a little nearer the blue. The similarity of these two spectra is remarkably complete, for on increasing gradually the thickness of the solution the bands in the red are gradually hidden, and when the absorption has crept up to 600, then a band at 540 to 525 becomes visible. The spectrum is remarkable for its permanence, if the solution be either weak or strong, hot or cold, the spectrum remains unchanged. This agrees with what one would naturally expect, that, under these circumstances, the cobalt remains as chloride. Moreover, from the similarity of this spectrum with that obtained with the fused chlorides, the inevitable conclusion is that the cobalt is in the same state in both cases.

This spectrum of cobalt chloride is very characteristic in its appearance and very easily produced; without special care and with a common spectroscope ·0005 grm. of cobalt chloride can easily be detected, and even the presence of other coloured chlorides does not interfere with it; for instance, even the presence of iron, nickel, chromium, manganese, does not hide the characteristic appearance produced even by the above small amount of cobalt chloride.

Supposing the 532 band be produced by a specific compound, it seemed likely that at least a rough estimate of the amount of that body, present under different circumstances, might be judged, by bringing always the absorption in the red up to the same point and comparing then the intensity of this 532 band; practically, however, this did not lead to results of any value.

It is, however, important to note that the spectrum of the fused chloride is never seen without the 532 band; and that in the other two cases already described, this band is only visible when a considerable amount of the cobalt salt is present. It seemed, therefore, possible that since the amount of the 532 producing body appeared, judging very roughly by the eye to diminish on dissolving the solid chloride in hydrochloric acid, that in an excessively dilute hydrochloric acid solution this substance might be destroyed and the band have disappeared. Experiments, however, did not confirm this, for when a solution so dilute that it required 6 feet of the liquid to bring the absorption up to 600, the 532 band was just as visible as in a strong solution. To sum up, with regard to this spectrum it is remarkably persistent; strengthen the solution or increase the length of the column looked through, or raise the temperature of a dilute solution, and the effect is the same; first, the shaded part up nearly to 650 becomes uniformly dark, then the absorption closes in as far as the 610 band, and the absorption creeps on up to 600; immediately after reaching this point the 532 band becomes visible.

Still further increase the absorption and a shadow is seen extending from the 532 band towards the red to 560. And lastly, with the strongest solution that can be looked through, there is still discernible a broad black band from 550 to 520, and the absorption from the red end reaches now to 565, and it is remarkable that in the ultra-red, where at first there was complete absorption, now light is transmitted. At the blue end considerable absorption occurs, no light being visible beyond 480.

Ethylic, methylic, and amylic alcohols, the saline ethers, and glycerine all dissolve cobalt chloride freely, and on being saturated with anhydrous chloride of cobalt give this same spectrum (fig. 3). These liquids must, of course, be free from water, or the true spectra are complicated by the spectra of the hydrates.

When ordinary alcohol is the solvent, a very large amount of the chloride is taken up and a mere film of the liquid on a microscopic slide is quite sufficient to give clearly the characteristic spectrum (fig. 3).

With amylic or methylic alcohol no appreciable alteration in the position of the bands takes place; but, with glycerine, both the bands have moved slightly towards the red.

That solvents so different as the above should all give the same spectrum with the cobalt chloride seems only to be explainable on the view that the spectrum so formed is that of the chloride itself, and not that of any new combination, in fact, that the solvent has so far altered the molecular structure of this salt that in place of its giving the spectrum (fig. 1) which it does when alone and solid, it gives this second spectrum (fig. 3); and further, it seems, whether the chloride be in the liquid or solid state, whether dissolved in alcohol or in a solid alkaline chloride, the spectrum is practically the same.

Although not probable, at the same time it seemed possible, that the identity of these different spectra might be due to a trace of water in each of the solvents, and that this banded spectrum was really that of a hydrate, and not of the chloride. To settle this point the following experiments were carefully carried out: Some of the mixed chloride of cobalt and potassium was dried and fused in a glass tube in a slow current of very thoroughly-dried carbon dioxide, in the first instance for twenty minutes; it was then allowed to cool and was spectroscopically examined, and found to be quite unchanged; again the heating and the slow passage of dry gas was continued for forty minutes, and afterwards for two hours, but in no case was the least change apparent, the spectrum-giving substance was evidently as abundant after all the heating as before, and no trace of any other spectrum was visible. The experiment was also made of fusing separately in different parts of same tube, filled with dry carbon dioxide, the cobalt chloride, and potassium chloride, and, after they had been in a state of fusion for some time, allowing them partially to mix, and sealing up the tube; where the two had come into contact this spectrum was visible and nowhere else. The hydrochloric acid solution of the cobalt chloride in one respect, however, is different from any of the other solutions, for, as before mentioned, only one spectrum is obtainable from it, whereas with the other solvents, the spectrum varies with the amount of chloride in the solution. Thus a very dilute alcoholic solution gives a spectrum (fig. 7) evidently different from the former one given by the saturated solution. To obtain clearly this second spectrum dry alcohol must be used, and should contain about 0·008 grm. of cobalt chloride in 100 cub. centims. of the alcohol; this amount of the cobalt salt gives only a faint purple colour to the alcohol, and requires a solution of about 2½ inches in depth to show the spectrum. If the strength of the solution be gradually increased so that it becomes of a strong blue colour, and contains about 20 grms. of the chloride in 100 cub. centims. of alcohol, and requiring for observation only about one-eighth of an inch thickness of the solution, a third spectrum (fig. 8) is obtained. This, however, appears in part to be a compound spectrum, formed by the presence in the solution of the bodies which produce the former spectra. Indications of two bands, 610 and 625, for instance, are visible, but what is characteristic of this spectrum, and does not occur when the solution is very strong or very weak, is the band at 585. In order to determine whether any other changes would occur when the solution was still more dilute, some of it requiring a depth of 2 inches to show the spectrum was diluted to such an extent that 6 feet of the liquid was required. This great dilution produced no change whatever in the spectrum. Other liquids, such as dry amylic alcohol or amylic acetate, which readily dissolve cobalt chloride, give spectra corresponding to those given by ethylic alcohol. With methylic alcohol, however, the first or most dilute stage was not obtained, but this evidently arose from a trace of water adhering most persistently to the alcohol. If liquids be used in which the cobalt chloride is not so soluble, dry acetic acid for instance, it will be found that only two spectra are produceable, corresponding to those in the more dilute alcoholic solutions; and with a liquid in which the cobalt chloride is still less soluble, such as dry ether, there is only one spectrum obtainable. And lastly, with such a liquid as tetrachloride of carbon or benzol, in which the cobalt chloride is absolutely insoluble, the spectrum, which is faintly visible when the liquid and the salt, in a fine state of division, are shaken up together, is that of the pure fused chloride. The action of heat on these solutions is two-fold: first, it increases the amount of colour; this arises, no doubt, to a considerable extent, if not altogether, from an absolute increase of the amount of anhydrous cobalt chloride present, for if a mere trace of water be present, on adding the blue cobalt salt the first action is for it to combine with the water, and to produce a pink and comparatively colourless solution; this hydrate, as will be shown further on, is decomposed on rise of temperature, giving the blue chloride. The second action of heat is to transform the saturated solution spectrum or third stage, into the intermediate or second stage, and the intermediate one into the first—the one given by most dilute solutions. Again, in the spectrum of the third stage, the 532 band is visible, but with the alcoholic solution the absorption has to extend nearer to the blue to about 575 instead of only 600, as with the hydrochloric acid solution, before this band becomes visible. This seems to indicate that less of the body producing it is present in the alcoholic than in the hydrochloric acid solution.

When water is the solvent for the cobalt chloride, the phenomenon is somewhat more complicated, as direct combination occurs and hydrates are formed. The colour of the dilute solution, as is well known, is pink. If this be examined with the spectroscope no sharp bands are visible; there is, however, very considerable absorption at the blue end of the spectrum, but with certain concentration and depth of solution, it is easy to distinguish a wide absorption band extending from 550 to 485 (fig. 16), and beyond this blue rays are comparatively freely transmitted. This band, however, is never sharply defined, but shades off on both sides; it undoubtedly belongs to certain hydrates of the cobalt chloride. To study the formation of these hydrates, solutions containing different amounts of cobalt chloride were prepared and tested as follows: first, a solution containing only 0·1 grm. of cobalt chloride in 100 cub. centims. of water was made, and the spectrum it gave was compared with that of a solution containing 100 times as much cobalt chloride. These solutions were found to give identical spectra. Again, 15 inches of a 2½ per cent. solution gave the same spectra as 4½ inches of the 10 per cent. solution, and 16 inches of 1 per cent. solution was compared to 1½ inch of 10 per cent. solution. So that in all these dilute solutions, as far as the spectroscope can tell us, there are the same compounds present. With stronger solutions this is not the case; for if the solution contains, say, from 20 to 30 grms. in 100 cub. centims. of water, and a thin layer of it be compared with a thick layer of a more dilute solution, the spectra will be seen to be essentially different. This new spectrum, which proves to be a very interesting one, is best seen when a saturated or nearly saturated solution of cobalt chloride is used (100 cub. centims. at 16° dissolve 32 grms. of cobalt chloride), then there are two bands—one at 625 and the other at 610—and an absorption stopping sharply at 652. Evidently we have again the spectrum of the dissolved cobalt chloride, a spectrum which has already been shown to be perfectly independent of water, consequently it appears that the anhydrous cobalt chloride is capable of existing in an aqueous solution.

If a solution saturated at ordinary temperatures with the chloride of cobalt be diluted with half its volume of water, the bands at 625 and 610 disappear; but if to this solution, or even to one more dilute, any substance be added capable of combining with water, such as sulphuric acid, calcium chloride, or even ammonium or sodium chloride, the bands at 625 and 610 again appear.

Heat acts on these dilute solutions in the same kind of way. If, for instance, this saturated solution, diluted with half its bulk of water, be heated up gradually at 49°, the 625 band first becomes visible, and then the 610 band. As the temperature increases the absorption in the green diminishes, and at the boiling point you have a spectrum very similar to that obtained by fusing together chloride of cobalt and chloride of potassium.

The reverse action also occurs. A solution giving strongly the bands at ordinary temperature ceases to show them when cooled. In both cases as the solution returns to its original temperature so does the original spectroscopic appearance return. In many cases the spectroscopic variations by temperature are very delicate and very striking in appearance. Any change tending to break up the hydrates seems always to diminish the amount of absorption taking place towards the blue end of the spectrum. The well known change of colour of strong cobalt solution on heating bears this out. Pass hydrochloric acid gas, for instance, into a dilute cobalt solution, and the gradual formation, and the chloride spectrum and disappearance of the absorption in the green and blue, is well shown.

In proportion as one compound, the hydrate, ceases to exist, so the other, the anhydrous compound, is apparently produced. The ordinary crystals of chloride of cobalt contain six molecules of water, and they give a spectrum shown at fig. 9. There are certain cases in which the hydrated crystals and their aqueous solutions give similar spectra; but with cobalt chloride this is not the case, the hydrate is not the one existing in the solution. The readiness with which the pink crystals of the hydrated chloride become blue arises from the ease with which it parts with water, the blue compound in all cases giving the spectrum of the anhydrous chloride.

There is another band which is generally visible in moderately strong aqueous solutions at 653 to 638, and is often well seen with a depth of about 3 inches of a 10 per cent. solution. This band, which, from its origin and its constant occurrence appeared to belong to all the cobalt salts, is really due to the presence of nickel. Cobalt salts which have been carefully purified do not give this band, even when very strong solutions are used.

The oxide of cobalt is spectroscopically an interesting body. It gives, as is well known, a very characteristic spectrum (fig. 10). This is readily obtained by examining the precipitate formed on adding an excess of caustic potash or soda to a solution of any cobalt salt. This blue precipitate (which is said to be a basic oxide) rapidly loses its colour, and the spectrum disappears. The spectrum consists of two very strongly marked bands, one about 665 to 638, and the other from 600 to 585. These are very characteristic of the oxide, for on increasing the thickness of absorbing layer, these bands always join together, forming thus a single band, before they increase in other directions. Besides these two bands, when the oxide is precipitated by potash or soda, there is always an absorption at the blue end, beginning at about 550, and having very much the appearance of a band, but rendered somewhat indistinct by the general absorption taking place in the blue. At the red end of the spectrum the absorption ends at about 675. If ammonia be used as the precipitant in place of potash or soda, then there is clearly no band in this part of the spectrum and no absorption at 540 (fig. 11). This difference between the two spectra seems clear and definite, and is of much interest, and the obvious deduction is that the two bands at 640 and 585 are produced by the oxide, and that the absorption at 540 is produced by a compound of oxide and potash or soda.

Vogel has pointed out the similarity existing between the spectrum of the precipitated oxide and that given by ordinary cobalt glass. The spectrum of the glass is seen at fig. 10A, and it will at once be seen how nearly this spectrum agrees with that of the oxide precipitated by potash or soda; the two bands are of the same character as before and very nearly in the same position, but apparently a little nearer to the red, and now clearly a third band from 550 to 525 is visible. The conclusion seems inevitable that the oxide and potash compound which exist in the precipitated oxide exist also in the blue glass. Again, these same spectra can be obtained by another process, and it is easy to prepare a blue liquid which gives an absorption spectrum identical with that produced by a piece of blue glass.

Winkler showed some years ago that when finely divided metallic cobalt, oxide of cobalt, or a salt of cobalt was boiled with caustic potash, a blue solution was formed, which he attempted to show contained cobaltate of potash. To the acid forming oxide he at first gave the formula CoO_5, and afterwards that of CoO_3. Schultze, however, immediately repeated Winkler's experiments and came to the conclusion that instead of its being a solution of cobaltate of potash, it was merely an alkaline solution of cobaltic hydrate. The absorption spectrum seems clearly to show that the blue liquid is not a new cobaltic salt, but that it contains essentially oxide of cobalt in solution and the same potash or soda compound as exists both in the glass and in the precipitated oxide. Again, it may further be deduced from the similarity of these spectra that the blue precipitate is not a hydrate, for it gives the same spectrum as exists in the glass, or at all events the water can only be so feebly combined with the oxide as not to alter the individuality of the compound. Again, if oxide, or, in fact, any ordinary salt of cobalt, be dissolved by fusion in borax, a glass which gives the same spectrum as ordinary blue glass is obtained. The positions of the bands, however, are apparently intermediate in position between those in the glass and those in the precipitated oxide; these differences are, however, very small.

The blue oxide of cobalt can be made to undergo another change which is of much interest. If a solution of cobalt chloride be precipitated by caustic alkali, taking care to keep the cobalt chloride in excess, thus on well agitating the precipitate and solution together, or even on slightly boiling them, the precipitate will be found to be of a green colour, and to contain, as an integral part, cobalt chloride. Evidently an oxychloride has been formed. Fig. 12 gives the spectrum produced in this way; it is a mixed spectrum of oxychloride and of oxide. The same body may also be obtained if the blue oxide be first prepared, drained rapidly and washed and then shaken up in a solution of the chloride. The stronger the solution used the more chloride attaches itself to the oxide, and apparently one or more oxychlorides are formed. If any other salt than the chloride or bromide and probably the iodide of cobalt, be used and precipitated by a small amount of caustic alkali, no salt corresponding to this oxychloride is formed, but the oxide is at once thrown down. This green substance is but very slightly decomposed by cold water and not at all by alcohol. A sample of washed blue oxide was divided into three portions: one was shaken up with a very dilute solution of cobalt chloride, another with a solution made by adding 3 cub. centims. of a saturated solution of chloride to 50 cub. centims. of water, and the third portion with a saturated solution of the chloride; the green compound formed in each case was dried out of contact with air, and the chlorine it contained determined; the first one contained chlorine corresponding to 4·85 per cent. of cobalt chloride, the second with 10·94 per cent., and the last with 28·02 per cent. of the cobalt chloride. The compound having the formula $4CoOCoCl_2$, would contain 30·40 of cobalt chloride. Such are chemically the changes which occur. Spectroscopically, the changes which the oxide undergoes are shown at figs. 13, 14, and 15. Fig. 10 being the spectrum of the original blue precipitate.

These changes are, first, the fading of the band at 590, and then the disappearance of all absorption on the less refrangible side of 590, and ultimately the formation of a dark and well-defined band from 674 to 665, the faint band from 600 to 596 remaining. This change has much the appearance of a division of this oxide band into two bands of which the most refrangible part from 590 to 580 remains, and the less refrangible part from 590 to 600 disappears. It seemed possible that with a spectroscope of greater dispersive power to separate the 690 band in the oxide into these two bands, but on trial this did not take place.

This spectrum, fig. 14, of the oxychloride, is in many respects similar to the spectrum of second stage of the chloride in alcohol, although certainly not identical with it, but in both cases the chloride appears to be modified somewhat in the same way. That it is not an oxychloride which is formed in the alcohol solution, seems proved by the fact that as previously pointed out, this spectrum is formed by dissolving the chloride in acetic acid.

Warm water decomposes this green compound, washing out the cobalt chloride. This decomposition is very clearly shown in the absorption spectrum, if, for instance, some of the green compound be prepared by precipitation as before described, and a little of it be placed on a microscope slide under a covering glass, it gives very distinctly the spectrum shown at fig. 13. Now wash the precipitate on a filter with warm water, and then, on taking another sample of it, the two bands at 647 and 592 will be distinctly visible, that is, there is evidently now oxide present. This oxychloride of cobalt may also be formed in small quantities by digesting the precipitated oxide in a solution either of nickel or potassium, or other chlorides, for on examining the oxide thus treated, the change of the absorption spectrum is distinctly visible.

The bromide of cobalt appears to react with the oxide in the same way as the chloride.

Generally the soluble cobalt salts of the oxygen acids do not give band spectra, but only a broad absorption shading off on both sides and extending from about 550 to 480 (fig. 16), and a considerable amount of general absorption at the blue end of the spectrum; for instance, the sulphate, nitrate, and acetate all act in this manner. An interesting reaction is visible with the cobalt carbonate. It is stated in works on chemistry that on adding sodic or potassic carbonate to a cobaltous solution it is not the normal carbonate, but a mixture of the carbonate and hydrated cobaltous oxide, that is formed.* If now this precipitation be made in front of the spectroscope, the changes which go on are perfectly visible. On the addition of the alkaline carbonate a precipitate forms which gives no bands, but after a very short time, perhaps half a minute or less, the bands of the oxide become distinctly visible; apparently then it is a pure carbonate which is thrown down, but that it rapidly, by the action of the water, undergoes decomposition, and oxide is formed.

Under special conditions, however, some of the oxygen salts may be made to give band spectra, for instance, on fusing chloride, oxide, or phosphate of cobalt with microcosmic salt and allowing it to cool, a pink-coloured glass is obtained, which gives a spectrum consisting of bands with much general absorption. Since all three of these cobalt compounds give under these circumstances the same spectrum, it apparently leaves little doubt that it is the spectrum of a phosphate of cobalt. If this pink bead be heated, it undergoes an interesting change, the absorption in the blue entirely disappears, and you get a spectrum identical with that of the oxide; for instance, that of an ordinary borax or glass bead (fig. 10). As this phosphate glass cools it again becomes pink, and the banded spectrum returns; evidently the phosphate is dissociated, recombination taking place as the temperature falls. When sodic phosphate is added to a cobaltous solution, a precipitate is formed which is soluble in excess of the cobalt salt; on continuing the addition of the phosphate, first a permanent pink precipitate is obtained, and afterwards when the phosphate is in excess the precipitate is of a blue colour. If the cobalt salt be added to the disodic phosphate, the blue precipitate forms at once. Now, on examining this blue precipitate spectroscopically, it is at once evident that oxide of cobalt must be present, for there is exactly the same spectrum as is given by the precipitate formed on adding caustic alkali to a solution of cobalt. The pink precipitate gives a faint band about 590, but the bands seen in the fused phosphate were not visible in the precipitate.

The foregoing account of these different spectra is but very imperfect; many most interesting questions with regard to the nature and meaning of the spectra have to be left for the present unsolved for want of sufficient data. Further experiments with other compounds are required, and above all it would be of the highest interest to investigate fully the parts of the spectrum not visible to the eye;

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* Miller's "Chemistry," p. 536.

evidently with the cobalt salts many, and it may be the most important, bands lie in the ultra-red. All the above spectra and many more have been drawn from direct and repeated observations, but obviously it is not intended to claim any originality for them; many have been described before.

My best thanks are due to my assistant, Mr. W. Lapraik, for the help he has given me in carrying out these investigations, which have extended over a considerable length of time.

"On the Female Organs and Placentation of the Racoon (*Procyon lotor*)."

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[PLATES 3—6.]

Of late years our knowledge concerning the form and structure of the placenta, as well as of the arrangement of the fœtal membranes in various species of mammals has been much extended through the researches of Milne-Edwards, Ercolani, Turner, and others. The observations of these anatomists have not been confined to the placentation of any one group, but extend to that of species belonging to every Mammalian order. Among these orders that of the Carnivora has received a not inconsiderable share of attention, not only from the observers just named, but also from those among whom may be mentioned the names of Daubenton, Von Baer, Bischoff, and others who appeared earlier in the field. Among the members of the fissiped group of Carnivora, the placenta or fœtal membranes, or both, have been described in the marten (*Martes domestica*),[*] stone marten (*Mustela foina*),[†] pine marten (*Mustela martes*),[‡] weasel (*Mustela vulgaris*),[§] ferret (*Mustela furo*),[||] bitch (*Canis familiaris*),[¶] fox (*Canis vulpes*),[**] cat (*Felis domestica*),[††] lioness (*Felis leo*),[‡‡] hyæna (*sp. ?*),[§§] and otter (*Lutra vulgaris*),[||||] whilst among the pinnipeds we find that the placentation has been examined in *Phoca vitulina*,* *Phoca bicolor*,† *Halichœrus gryphus*,‡ as well as in two seals, the species of which were not determined.§

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[*] Buffon's "Histoire Naturelle," vol. vii, p. 81, Pl. XX.

[†] Bischoff, "Sitzungsber. Akad. Wissensch.," München, May 13, 1865.

[‡] *Idem*, p. 343.

[§] *Idem*, p. 343.

[||] Buffon's "Histoire Naturelle," vol. vii, p. 105, Pl. XXVII.

[¶] Bischoff, "Entwickelungs-geschichte des Hunde-Eies." Von Baer, "Entwickelungs-geschichte der Thiere," 2ter Theil, p. 238.

[**] Turner, "Trans. Roy. Soc. Edin.," vol. xxvii, p. 297.

[††] Turner, "Lectures on the Comparative Anatomy of the Placenta," p. 72.

[‡‡] Owen, "Anatomy of Vertebrates," vol. iii, p. 744.

[§§] *Ibid.*, p. 744.

[||||] Bischoff, "Sitzungsber. Akad. Wissensch.," München, March 4, 1865, p. 213.

It will be observed that in this list, which, so far as I can ascertain, contains the names of all the carnivorous mammals the placentation of which has been investigated, that of the racoon (*Procyon lotor*) does not occur. This is the more remarkable, inasmuch as the racoon is one of the mammals which most frequently finds a place in our collections, and one which apparently thrives well under artificial conditions of existence. At the same time, as occurs so frequently in the case of wild animals kept in a state of captivity, the generative functions of the racoon appear almost constantly to remain in abeyance. Mr. Bartlett, the skilful superintendent of the menagerie of the Zoological Society of London, however, in answer to enquiries, informs me that the racoon has occasionally bred in confinement, but only after the mother had been in the gardens for several years. He adds, moreover, that neither the length of the period of gestation nor the condition of the young at birth are known to him. In view of the total lack of definite knowledge concerning the anatomy of the female organs of *Procyon* in the pregnant state, I consider myself extremely fortunate in being able, owing to the kindness of the Messrs. Jennison, proprietors of the Bellevue Zoological Gardens of this city, to give some account of these organs as they occurred in an individual which, in their gardens, had reached an advanced stage of pregnancy.

I shall, in the first place, describe the female organs, and thereafter proceed to the consideration of the anatomy of the placenta.

On opening the abdominal cavity, the distended condition of the right cornu uteri showed at once that the animal was pregnant.

Ovary.—The ovary, rounded in form and somewhat flattened from above downwards, measures one-fourth of an inch in diameter. Its surface presents a somewhat lobulated appearance, which, however, is not due to the presence of corpora lutea, not a trace of these being distinguishable in the organ of either side. The ovary is situated in close proximity to the free extremity of the uterine horn, and is retained in position by means of two ligaments. Of these, one, to be more particularly described presently, attaches it to the posterior surface of the diaphragm, whilst the other passes from the inner end of the ovary to the free extremity of the corresponding uterine horn. The Fallopian tube, moreover, almost encircles it and serves to retain it in close relation to the uterus. The ovary is altogether destitute of any peritoneal pouch or pavilion, such as in many animals, *e.g.*, hyæna, forms an almost complete sac for the accommodation of this organ.

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* Barkow, "Zootomische Bemerkungen," Breslau, 1851.

† Alessandrini, "Meckel's Deutsch. Archiv für Physiologie," vol. v, p. 604.

‡ Turner, "Trans. Roy. Soc. Edin.," vol. xxvii, p. 275.

§ Rosenthal, "Nova Acta Acad. Cæs. Leo. Car.," vol. xv, 1825.

Fallopian Tube.—This tube describes a very peculiar course. Its abdominal opening, or morsus diaboli, lies in relation to the dorsal surface of the ovary, and is placed between the inner end of the latter and the free extremity of the uterine horn. The opening itself is directed upwards toward the spine of the animal, and appears, at first sight, to be altogether destitute of fimbriæ. Upon closer examination, however, with the aid of a magnifying glass, a number of very minute fimbriæ are seen to surround the aperture. These fimbriæ do not project beyond the margins of the opening, but are concealed within the orifice, and consequently can be of no service at the time of ovulation in grasping the ovary and bringing the latter into direct relation with the Fallopian tube. From the morsus diaboli the tube winds forward so as to reach the anterior margin of the ovary along which it passes as far as the outer extremity of the latter. Here it changes its course, and becoming applied to the posterior border of the ovary, extends along it and terminates abruptly in the much wider extremity of the uterine horn. The Fallopian tube thus almost encircles the ovary. The tube measures three-fourths of an inch in length and one-eighth of an inch in diameter. It is of uniform diameter throughout and presents no trace of a dilated extremity or so-called infundibulum.

Exterior of Uterus, Vagina, and Urogenital Canal.—The uterus consists of a central body and two horns. The left or unimpregnated horn was flattened from above downwards, and was strongly curved, the convexity of the curve being directed forwards. Measured from the middle line of the uterus, its anterior or convex border amounted to 1¾ inches in length, whilst its breadth, which was almost uniform, amounted to half an inch. Its free extremity was rounded and into it the Fallopian tube opened abruptly. The right or impregnated horn measured 5¼ inches along its convex margin, whilst its posterior, or attached margin, equalled that of the horn of the opposite side. It will be observed, therefore, that after impregnation the alteration in size of the uterus takes place in respect of the anterior or free margin of the impregnated horn, the posterior or attached border of the latter remaining of the same dimensions as that of the opposite side.

The impregnated horn measured 2 inches in greatest breadth and presented a somewhat globular form. Subsequent to injection of the uterine artery with carmine, numerous extremely tortuous vessels could be readily distinguished through its peritoneal investment. These vessels, which, upon opening the uterus, were seen for the most part to correspond to the place of attachment of the placenta, were mostly directed transversely to the long axis of the horn, and extended from the attached to the free border of the latter. They anastomosed freely with one another and formed a wide arterial meshwork on both the upper and lower surfaces of the horn. It was, however, observed that none of the vessels of the one surface extended over the anterior border of the uterus so as to come into relation with those of the other surface, and that, consequently, no anastomosis could be distinguished between them. It is probable, however, that the failure to trace any distinct anastomosis between the vessels of the two surfaces of the impregnated horn ought to be attributed to the incomplete nature of the injection, as it appears unlikely, in view of the continuity of the placental substance between these surfaces, that any break in the continuity of the vessels of opposite sides really existed. The vessels in question were visible over the entire surface of the impregnated horn, but were much more thickly distributed in its middle third, which corresponds to the placental zone.

The corpus uteri is not differentiated externally from the vagina, the two organs forming a continuous tube. The position of the os uteri, however, is clearly defined in the interior of the organ by a well-marked valvular fold, to be afterwards described. Taking into consideration the position of this fold, the body of the uterus measures 2½ inches in length and half an inch in breadth. It is flattened from above downwards, and is attached to the walls of the pelvis by means of the broad ligament. This ligament is formed in the usual manner of a double fold of peritoneal membrane, which, after covering the whole of the upper surface of the uterus, and even the anterior portion of the corresponding surface of the vagina, is reflected upon the rectum. The lower surface of the uterus is not invested to the same extent by peritoneum, that membrane being reflected upon the bladder before reaching the os uteri. The uterus, moreover, is retained in position by two additional ligaments, the diaphragmatic and the round. The former consists of a stout bundle of involuntary muscular fibres, which, passing backwards from the diaphragm to free extremity of the corresponding uterine horn, extends inwards parallel to the posterior border of the latter and is inserted into the body of the uterus. The ovary is fixed to the lower surface of this ligament, close to the free extremity of the uterine horn. The *round ligament* is also well developed. It is attached by one extremity close to the free end of the cornu uteri, and passes backwards to the inguinal region, where it terminates in the usual manner.

The vagina, from the os uteri to its junction with the urethra, measures 1½ inches in length. *Externally* it is not differentiated from either the corpus uteri or the urogenital canal. The latter measures 2 inches in length. Its external orifice is bounded by two laterally placed folds, or labia, which together constitute a conical eminence measuring half an inch in height, upon the summit of which the external genital orifice is situated.

Bladder and Urethra.—The parts having been immersed for some time in spirit, the bladder was to a certain extent contracted. It measured when distended 1½ inches in length and was of a regularly oval form. The muscular coat was extremely thick, the separate bundles of muscular fibres being clearly defined beneath the peritoneal investment. The latter covered the whole of the upper surface of the viscus as far back as the entrance of the ureters, as well as the upper portion of its lateral surface. The lower surface of the apex of the bladder was also covered by peritoneum, whence that membrane was reflected to the anterior abdominal wall. The ureters entered the bladder half an inch from the neck, whilst the urethra passed off from the posterior extremity of the organ. The urethra measured 1¼ inches in length. It passed backwards, lying in the substance of the lower vaginal wall, and opened into the commencement of the urogenital canal in the usual manner.

Interior of Uterus, Vagina, and Urogenital Canal.—On slitting open the left or unimpregnated horn of the uterus, a well-defined septum uteri was seen to separate the right and left horns. It measured three-quarters of an inch in length, and extended backwards from the anterior or free margin of the corpus uteri. It was attached to the uterine walls in front, above, and below, whilst posteriorly it presented a free, concave, sickle-shaped margin which projected into the cavity of the uterus.

The muscular walls of the unimpregnated horn were of great thickness, and its mucous membrane, which was of a dense leathery consistence, was thrown into numerous thick fleshy folds arranged parallel to one another and at right angles with the long axis of the horn. These folds were present throughout the horn, with the exception of a space of half an inch in length next the septum uteri, at which place the mucous membrane was perfectly smooth and devoid of rugæ. On the left side of the septum uteri there was a single longitudinal fold of mucous membrane which extended from the attached to the free margin of the latter. There was no trace of fluid in this horn. The *transverse* arrangement of the mucous folds in the uterine horn of the racoon seems to be somewhat uncommon, these folds in the majority of mammals being longitudinal and not transverse in direction. According to Cuvier, however, a similar transverse arrangement of the mucous rugæ is met with in the uterus of the civet cat.*

On opening the right or impregnated horn its walls were observed to be much thinner than those of the opposite side. This diminution in thickness involved both the muscular and mucous coats, and is doubtless to be attributed to mechanical distention of the horn by the contained ovum. The mucous membrane presented no trace of the well-defined transverse rugæ met with in the horn of the other side, and was of a softer and more succulent texture. On careful examination, however, this membrane was seen to be thrown into numerous very minute tortuous and anastomosing folds, which closely resembled those described and figured by Professor Turner in the impregnated uterus of *Orca gladiator*.* These folds were more distinctly marked at the placental site than in other parts of the horn, where the mucous membrane was almost smooth. The more minute description of the microscopic structure of the uterine mucous membrane, together with the modifications which it undergoes in the impregnated horn, will be deferred till after the consideration of the ovum and placenta.

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* "Leçons," vol. v, p. 149.

Although the corpus uteri is not distinguishable externally from the vagina, yet upon examining the interior of the cavity the position of the os uteri is seen to be clearly defined by a well-marked valve-like projection of the mucous membrane. This valve consists of two segments, an anterior and a posterior, which, however, are continuous with one another, so that the entire structure presents an annular character. The central portion of each segment of the valve measures half an inch in breadth, whilst laterally, where it becomes continuous with its fellow, each segment diminishes to quarter of an inch in breadth. The two segments together form a circular valve attached to the entire circumference of the uterus, and prolonged superiorly and inferiorly into two points which project backwards into the lumen of the vagina. The mucous membrane of the body of the uterus is thrown into slight longitudinal rugæ, which commence opposite the free margin of the septum uteri, and extend along the whole length of the organ.

The diameter of the vagina, as already mentioned, is the same as that of the corpus uteri; and although, therefore, there is no distinct line of demarcation between these two portions of the genital apparatus visible externally, an examination of the interior of the organ shows that the limits of both are clearly enough defined. In fact, the separation of the corpus uteri from the vagina is indicated by the annular valve above described as corresponding in position to the os uteri; whilst, posteriorly, the vagina is distinguished from the uro-genital canal by the position of the urethral orifice, as well as by a slight fringe-like fold of mucous membrane, which is attached to the entire circumference of the vaginal canal. The mucous membrane of the latter is thrown into slightly marked longitudinal rugæ which, however, are of larger size than those met with in the body of the uterus.

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* "Lectures on the Comparative Anatomy of the Placenta," p. 45.

The calibre of the urogenital canal equals that of the vagina, the separation of the two organs being marked by the orifice of the urethra and by the fringe-like fold of mucous membrane just described. The mucous membrane of the urogenital canal does not differ in character from that of the vagina.

Having now described the female organs, I pass to the consideration of the

Ovum.

On opening the gravid uterine horn by means of an incision carried along its anterior or convex margin, a single fœtus was observed to occupy the greater part of the cavity. The fœtus itself, covered only by an epitrichial membrane, was at once in part exposed, from which it was evident that before coming into my hands the fœtal membranes had been ruptured. These membranes, although ruptured, where they lay in contact with the posterior extremity of the fœtus so that the latter was in contact with the wall of the uterus, retained their relation with the anterior moiety of the embryo; and I was thus enabled to make out the arrangement of both the chorion and allantois. With regard to that of the amnion there still remains some doubt, but to that I shall refer presently. The fœtus, curled upon itself, lay with its head directed towards the free extremity of the uterine horn, and close to the opening of the Fallopian tube, being separated from the latter merely by the intervention of the fœtal membranes which covered it. The left anterior extremity was closely applied to the side of the chest, whilst that of the right side was strongly flexed at the elbow-joint, so that the palmar surface of the hand rested against the side of the head. The posterior extremity of the fœtus touched the septum uteri, and the hind legs, fully extended, stretched backwards towards the body of the organ. The tail, strongly flexed, occupied the interval between the hind legs. The dorsal surface of the fœtus was accurately adapted to the anterior or convex surface of the uterine horn, whilst the ventral surface was directed backwards towards the posterior or concave margin of the latter.

The position of the fœtus of *Procyon in utero* appears to be somewhat unusual. In the majority of mammals, according to Milne-Edwards,* the head of the fœtus usually presents at the os uteri;† whilst in *Procyon* the posterior extremity of the embryo occupied that position.

The fœtus was encircled by an annular or zonary placenta which corresponded to the middle third of the uterine horn, and measured one and a quarter inches in greatest breadth where it lay in contact with the dorsal surface of the fœtus, while opposite the belly of the latter the placenta did not exceed half an inch in breadth. The placenta had been detached from the uterus, a circumstance which I cannot sufficiently regret, as I was thereby prevented from observing by means of injection the exact relation which existed between the maternal and fœtal portions of the placenta. Although detached from the uterine wall, it was evident that no change had taken place in the position of the placenta with reference to either the uterus or fœtus, and consequently the somewhat exceptional position of the latter *in utero* can in no way be attributed to this circumstance. The placenta formed a complete ring, but at the centre of its widest part, *i.e.*, opposite the back of the fœtus, there was a spot similar to that figured by Daubenton* in the placenta of *Martes domèstica*, and described by Bischoff in that of *Lutra vulgaris*,† *Mustela foina* and *Mustela martes*,‡ where the substance of the placenta was deficient. This deficiency involved the entire thickness of the placenta, so that a probe could be passed from the uterine to the chorionic surface of the organ without injury to its substance. The absence of placental substance at this spot is of much interest in view of the observations of Bischoff upon the corresponding arrangement in *Lutra*, to which I shall afterwards refer. To the fœtal surface of this portion of the placenta the amnion was closely adherent. The entire uterine surface of the placenta, with the exception of a band along either margin, which measured one-eighth of an inch in breadth, presented a very regularly papillated appearance. The marginal bands were perfectly smooth and of a lighter colour than the rest of the organ. Subsequent examination showed that this smoothness was due to the presence of a layer of decidua (reflexa) which concealed the subjacent papillæ.

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* "Leçons sur la Physiologie," vol. ix, p. 591.

† Turner describes the fœtus as occupying this position *in utero* in the seal, "Trans. Roy. Soc. Edin.," vol. xxvii, p. 278; sloth, *ibid.*, p. 75; and lemur, "Phil. Trans.," vol. 166, p. 574.

Chorion.—I have already remarked that the fœtal membranes had been ruptured where they lay in contact with the posterior extremity of the embryo. In consequence of this I experienced some difficulty in the determination of their exact arrangement. With the view of completing this with greater exactitude, I removed the entire ovum from the uterus, and having carefully extracted the fœtus by dividing the umbilical cord, as well as the narrowest part of the placenta, I placed the latter, together with the fœtal membranes, in a vessel containing spirit. By floating out the membranes in this way I obtained the view represented in fig. 2. From this drawing it will be observed that a membrane which is continuous with each margin of the placenta is prolonged outwards from the latter, so as to form a barrel-shaped envelope in the interior of which the fœtus was contained. This membrane, which was undoubtedly the chorion, agrees with that described in the dog, cat, and other Carnivora, not only as regards its form, but in the fact that a few blood-vessels were prolonged outwards from its villous or placental portion to ramify in its non-placental area. Judging from the extent to which the uninjured pole of the chorion, which accommodated the anterior portion of the fœtus, was prolonged outwards from the placental margin, it did not appear probable that any portion of what (with reference to the fœtus) may be termed the posterior pole of that membrane, had extended into the non-impregnated horn of the uterus. This point, however, in view of the lacerated state of the membranes, it was manifestly impossible to decide in the absence of another and more perfect specimen. At the same time it is to be observed that the limitation of the chorionic membrane to the impregnated uterine horn in the various species of Carnivora previously examined, justifies the conclusion that in *Procyon* that membrane does not extend into the unimpregnated horn, as happens in the case of the Ruminants, Solipeds, and others.

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* Buffon's "Histoire Naturelle," vol. vii, Pl. XX.

† "Sitzungsber. Akad. Wissensch," München, 11 März, 1865.

‡ *Ibid.*, 13 Mai, 1865.

Allantois.—With regard to the exact arrangement of this membrane, owing to the rupture which had taken place, I am by no means certain. At the same time I may state that, by means of careful dissection, I was able to detect a delicate membrane, which, although closely applied to the fœtal surface of chorion, could be readily detached from the non-placental area of the latter. This membrane, which I regard as the allantois, was coextensive with, and formed a lining to the uninjured pole of the chorion. At the margin of the placenta, the allantois could be traced onwards as a distinct membrane, which closely invested the fœtal surface of that organ. Moreover, an examination of the shreds of the posterior chorionic pole showed that here also the allantois could be detached in a fragmental form from the fœtal surface of the former. I feel justified, therefore, in assuming that, had the chorion been entire, the allantois would have been found to be coextensive with and to have borne the same relation to it that it bears in other Carnivora. I failed, however, to detect any prolongation of the allantoic sac along the umbilical cord, nor did I succeed in isolating as a distinct membrane that portion of the allantois which invests the outer surface of the amnion.

Umbilical Vesicle.—Of this structure I could not distinguish the slightest trace. Its absence in *Procyon* is remarkable in view of its constant occurrence in other carnivorous mammals.

Amnion.—After removal of the fœtus from the bag of the chorion, a third membrane was exposed, the interpretation of which occasioned considerable difficulty because of its peculiar relations to the other membranes already described. The appearance of the membrane is faithfully reproduced in fig. 2. The sac in question, when floated out in spirit, presented an appearance not at all unlike the finger of a glove.

Its one extremity was closed, and was attached to the central point of placenta, immediately over the dorsal surface of the embryo. It will be recollected that at this point the placental substance was deficient, and to the margin of this deficiency the blind end of the sac in question was closely adherent. The cæcal extremity, however, did not project through the aperture in the placenta, and consequently did not reach the uterine surface of the latter. A careful examination of the blind end of this sac showed that its outer surface was clearly continuous with that portion of the allantoic membrane which lined the fœtal surface of the placenta. Hence we conclude that in *Procyon*, as in other Carnivora, the reflection of the allantois from the inner surface of the chorion to the outer surface of the amnion takes place originally at the central point of the placenta, that is, from the margins of the placental gap; and that along with the amnion, although not separable as a distinct structure, there is combined the inner or reflected layer of the allantois. The other end of the sac was ruptured, and, extending backwards (with reference to the embryo), was compressed between the dorsal surface of the latter and the inner surface of the caudal pole of the chorion. When distended with spirit this sac proved to be of such a size that I had no difficulty in replacing the fœtus in its interior. Although from the ruptured condition and its abnormal position with reference to the embryo, the resemblance of this sac to that of the amnion was much obscured, I have little doubt that it must be regarded as representing the latter. In favour of this view is the fact that, unless it be so regarded, there was no other structure the arrangement of which was in any way comparable to that of the amnion; and conversely if it were not the amnion, and inasmuch as its relations negatived the supposition that it could be the umbilical vesicle, we should be compelled to assume that, in *Procyon*, we have a fœtal membrane of which there is no representative in any other Carnivore, the placentation of which has been hitherto described. Moreover, the close attachment of the blind extremity of this sac to the margins of the central deficiency of the placenta lends additional support to this view. Bischoff,* in his account of the placentation of the otter, refers to a corresponding deficiency in the placenta of that animal. A similar observation is recorded by him in respect of the placenta of *Mustela foina* and *Mustela martes*.† The presence of this gap in the placental substance he explains by supposing that the separation of the false from the true amnion takes place in these animals at a relatively later date than in the majority of mammals; and that, consequently, an obstruction is thereby occasioned to the investiture, by the vascular allantois, of the fœtal surface of the chorion, resulting in the deficiency in question. In accordance with this view, the placental gap must of course correspond to the central point of the placenta, inasmuch as at this point the separation of the true from the false amnion takes place. The close attachment of the true amnion to this point, as well as the fact that the continuity of the allantois over the junction of what originally was the false with the true amnion, could be clearly traced in *Procyon*, lends additional support to this view; while, at the same time, it strengthens the interpretation above given of the morphological significance of the sac in question.

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* "Sitzungsber. der Akad. der Wissensch.," München, 4 März, 1865.

† *Ibid.*, 13 Mai, 1865.

The Fœtus.

Upon removing the fœtus from the uterus, the former was seen to be completely invested by an opaque membrane of a bluish colour. This membrane, to which the name of epitrichium has been given by Welcker,* formed, as it were, a second or supernumerary cuticle to the fœtus, and was closely applied to every part of it. Over the greater part of the fœtus it lay superficial to and concealed the hair with which the embryo was covered. At some places, however, and more especially over the inter-scapular region, the forehead, and the outer sides of the thighs, the free extremities of the subjacent hairs projected through this membrane, as did also the large vibrissæ surrounding the mouth. The membrane was thinner and more closely adherent to the head, the distal half of the tail, and to the legs below the knee and elbow joints, than elsewhere, and could not be removed from these parts without tearing; whereas, from the other parts of the body, where the membrane was of greater thickness, it could readily be detached as an entire structure. The membrane agreed in all respects with the epitrichium described by Professor Turner in the two-toed sloth (*Cholœpus Hoffmanni*),† except that in the latter, with the exception of the tactile vibrissæ of the snout, none of the hairs protruded through this membrane; whereas in *Procyon* numbers of the hairs of other parts of the body pierced it. As in *Cholœpus Hoffmanni*, so in *Procyon*, the epitrichium was continuous with the mucous membrane of the ears, eyes, mouth, and anus. An examination of the deeper surface of the membrane disclosed a punctated and irregular appearance of the latter. This was doubtless due to the impact of the growing hairs. A similar appearance was met with by Professor Turner in the deeper surface of the epitrichium of the sloth. Under the action of caustic soda the membrane resolved itself into numerous flattened epithelial scales, in which at times distinct nuclei were visible. These scales differed from those described by Professor Turner in the sloth,‡ inasmuch as their edges were uniformly smooth, and presented no trace of the interlocking processes described by that anatomist.

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* "Abhandlungen der Naturforsch. Gesellschaft," Halle, Band x, p. 20.

† "Trans. Roy. Soc. Edin.," vol. xxvii, p. 71.

‡ *Ibid.*, p. 71.

A supernumerary epidermic covering or epitrichium is not confined to *Procyon.* A similar epidermic covering has been described by Welcker as investing the fœtus of *Bradypus*, *Cholœpus*, *Myrmecophaga*, *Dicotytes*, *Sus*, and *Equus*, and more recently Professor Turner has noted its occurrence in the fœtus of *Cholœpus Hoffmanni.* Welcker states that he found no trace of this membrane in the fœtus of *Felis* or *Ursus*. Its total absence in the latter, and its presence in a well-developed form in *Procyon* is remarkable, seeing that, in respect of so many important structural details, these two genera approach one another so closely. Further observations are necessary with regard to the distribution of this membrane among the members of the Carnivora, and when made will in all probability establish its existence in other genera than *Procyon*, in which alone it has hitherto been described.*

The fœtus, measured along the curve of the back from the tip of the nose to the root of the tail, was 4½ inches in length, whilst a straight line from the most projecting part of the forehead to the root of the tail measured 2½ inches. The dorsal region and sides of the body were covered with short hairs of a lead-blue colour, while on the forehead, lower surface of the neck, and outer sides of the thighs, the hair was of a lighter colour. On the ventral surface of the body and inner sides of the thighs the hair was not developed to the same extent as elsewhere. The nails were pure white and fully formed. The eyes were completely closed; and, looking at the comparatively mature condition of the fœtus, I am inclined to believe that, in the case of the racoon, as in that of some other Carnivora, the young are born blind. The umbilicus was situated three-fourths of an inch in front of the anus.

An umbilical cord, as such, could hardly be said to exist. The vessels composing it, after escaping from the belly of the embryo, almost at once separated, and applying themselves to the narrowest part of the placenta, extended forwards towards the back of the embryo, and, after giving off numerous arborescent branches, disappeared by dipping into the substance of the organ.

The vessels composing the cord consisted of two umbilical arteries and a single umbilical vein.

Microscopic Anatomy of the Placenta.

After the placenta had been removed from the uterus, its uterine surface was observed to present an appearance totally different from that which characterises the corresponding surface of that organ in either the bitch or cat, both of which I have myself examined, or indeed of that of any other carnivorous mammal whose placenta has been hitherto described. To the naked eye this surface presented merely a flocculent appearance, but when examined with the aid of a simple lens, or, still better, with the lower powers of the compound microscope (Ross, oc. A, object. 3-inch), this flocculent appearance resolved itself into a vast number of clearly-defined papillæ, which covered the entire uterine surface of the organ. The appearance presented under the microscope is faithfully reproduced in fig. 9. I have said that the papillæ covered the entire surface of the placenta; but, along each border of the organ, there was a narrow band, slightly raised above the level of the surface, where the papillæ in question at first sight appeared to be absent. Further examination, however, proved that the papillæ were present here as elsewhere, but that their presence was concealed by a superjacent layer of delicate tissue, which proved to be of the same structure as the uterine mucous membrane. This layer must therefore be regarded as representing the decidua reflexa. The papillæ, under the microscope, presented an appearance closely resembling that of the villi of the small intestine after a successful injection with opaque material, but were of considerably larger size. The papillæ were mostly club-shaped, the apex of each being of greater diameter than the base. Their summits were for the most part uniformly rounded, but here and there a papilla could be distinguished which presented a central depression on its free extremity. The separate papillæ were uniformly simple, and in no one instance did I observe secondary papillæ growing out from a primary stalk. I have observed that the separate papillæ resembled the intestinal villi, but the surface of the placenta, as a whole, presented a widely different appearance from that of the intestinal mucous membrane. This was due to the fact that each papilla was surrounded by a wall of placental substance, which, as it were, enclosed a space, in the centre of which the papilla was situated, and was thereby completely isolated from its neighbours. These walls were quite continuous all over the surface of the placenta. They were not so high as the papillæ which they surrounded; and, consequently, when the placental surface was viewed obliquely, the walls were hidden at places by the papillæ, and appeared to be absent. More thorough examination, however, disclosed their presence over the entire surface of the placenta. The spaces bounded by these walls varied in form. In some places they were square, at others five or six-sided, whilst occasionally they were circular. Looking at the placental surface as a whole, its appearance was not unlike that of a honeycomb filled with grubs, the walls corresponding to the partitions between the cells, whilst the papillæ very fairly represented the contained grubs.

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* For full details respecting the nature and distribution of the epitrichium the reader is referred to the papers already quoted of Welcker and Turner.

Towards the margins of the placenta the arrangement just described undergoes some alteration. In the neighbourhood of the band of decidua reflexa, the ridges became irregular, and were no longer distinguishable as continuous structures, but became broken up into more or less elongated portions, the free margins of which were provided with papilliform eminences. This gradual dismemberment of the continuous ridges could be clearly traced in passing from the centre towards the margins of the placenta, inasmuch as the ridges which were clearly continuous and regularly arranged, as above described, at one part of the placenta, became less and less regular and more tortuous at another part. The structure of these ridges, both at the central and at the lateral margins of the placenta, is the same, and differs widely from that of the papillæ, which they enclose. The papillæ I regard as belonging to the fœtal portion of the placenta, whilst the maternal portion is constituted by the ridges just described. Underneath the marginal band of decidua reflexa, the fœtal papillæ were of smaller size than elsewhere, and were arranged in tranverse tortuous rows, which corresponded in breadth to that of the decidual band.

The fœtal surface of the placenta, upon removal of the allantois, was seen to be divided into numerous minute polygonal areas which doubtless represent the so-called cotyledons described in the placenta of other Carnivora.

The placenta of the racoon presents no trace of the greenish coloration met with in the margins of that organ, in the case of the bitch,* cat,† and fox.† Neither is it provided with any pigment-containing sac similar to that described by Bischoff in the placenta of *Lutra*,‡ *Martes*,§ and *Mustela*.§

After removal of the placenta from the uterus, I attempted, by means of a pipe inserted into one of the umbilical arteries, to inject the fœtal vessels. In consequence, however, of the parts having been previously immersed in spirit, these were contracted to such an extent as entirely to prevent the passage of the injecting fluid. To this absence of injection was due the difficulty which I experienced in obtaining a really satisfactory demonstration of the arrangement of the placental blood-vessels in general, and more particularly of the relation which the maternal vessels bear to those of the fœtus. The elucidation of this point will only be accomplished by the examination of another injected specimen, but in view of the difficulty of obtaining such, I think it advisable meanwhile to communicate the results of my observations so far as they go.

When sections were made of the placenta, and the uterine margins of these were carefully examined under the higher powers of the microscope, there was observable a total absence of anything like a clearly-defined limitary membrane on the papillary surface. Consequently, in sectional views, the papillæ were indistinctly isolated from one another, although as observed in surface views, the separation was clearly enough defined. Here and there, but extremely rarely, an isolated columnar epithelial cell was seen to be attached by its narrow end to the papillar surface. Whether the uterine surface of the organ had been invested by a continuous layer of these cells, or whether those seen had been detached from the uterine mucous membrane, I was unable to decide. In view, however, of the fact that the uterine mucous membrane was entirely devoid of such cells, the latter supposition appears the more probable. The trenches between the papillæ, as well as those between the latter and the surrounding ridges above described, were, to a considerable extent, filled with numerous shreds of decidua, as well as by a quantity of blood corpuscles which presented various forms, due doubtless to the action of the spirit in which the parts had been preserved. It was particularly observed, that no vessel of large size, and presenting the structure which I believe to be characteristic of those constituting the fœtal portion of the placenta, extended beyond the uterine surface of the placenta. The marginal band of decidua reflexa presented the same structure as that of the mucous membrane of the impregnated uterine horn, to be afterwards described.

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* Von Baer, "Untersuchungen."

† Turner, "Lectures on the Comparative Anatomy of the Placenta," p. 71.

‡ "Sitzungsber. Akad. Wissensch.," München, 11 März, 1865, p. 218.

§ *Ibid.*, 13 Mai, 1865, p. 339.

The fœtal surface of the placenta was clearly defined in sectional views by the allantoic membrane. From this membrane numerous septa were given off at nearly regular intervals, and penetrated the substance of the placenta, so as to form, as it were, supports for the blood-vessels forming that organ. These placental septa could occasionally be traced as continuous structures nearly as far as the uterine surface of the placenta, but, for the most part, they disappeared before reaching the latter. In either case, they gradually became more delicate, and were finally lost to sight in the convolute of placental blood-vessels.

On proceeding to examine the blood-vessels composing the placenta, I found that these presented two distinct types of structure, according as I examined those which constituted the papillæ, or those which were met with in the circum-papillate ridges above described. In view of the facts which I am about to communicate, I regard the circum-papillate ridges as the torn margins of those processes of maternal tissue which interpenetrate the fœtal villi, whilst the papillæ themselves are simply the free extremities of the fœtal villi, which in the racoon, as in the bitch,* and cat,† reach the uterine surface of the placenta. The vessels met with in the maternal portion of the placenta resemble ordinary capillaries, whilst those which constitute the fœtal portion present an altogether different appearance.

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* Turner, "Lectures on the Comparative Anatomy of the Placenta," p. 82.

† *Ibid.*, p. 79.

Fœtal Portion of Placenta.—The fœtal vessels constituted by far the larger portion of the placenta. Indeed, when sections of the placenta were examined, the entire mass of the organ was apparently composed of these. This I afterwards found was due to the fact that the processes of maternal tissue in the absence of injection, could not be traced to any great depth in the substance of the organ, and that the delicate capillaries with which they were furnished, were with difficulty distinguished amid the much larger vessels of the fœtal portion of the placenta. The fœtal vessels resembled neither arteries nor veins in the structure of their walls, and yet were of much larger calibre than ordinary capillaries. So far as I am aware, vessels presenting a similar character have only once been previously noticed, and that by Professor Turner in the *maternal* portion of the placenta of *Cholœpus Hoffmanni.*[*] With the observations of that anatomist, with regard to the structure of these vessels, my own, except in one or two points, almost entirely agree. When a portion of the placenta of racoon was teased out under the microscope, the vessels in question were seen for the most part to run parallel with one another, from the fœtal to the uterine surface of the organ, and could be readily detached in the form of bundles from the cut edge of the placenta. When one of these bundles was farther manipulated, the separate vessels were observed for the most part to be of uniform diameter, although occasionally they presented well-defined enlargements. These in some instances occurred at regular intervals, and gave to the vessel a moniliform character. The vessels freely communicated with one another by means of transverse anastomosing branches of a similar diameter. These transverse anastomosing vessels were given off at somewhat irregular intervals, but for the most part they occurred at intervals which did not much exceed the diameter of the vessels themselves. In consequence of this arrangement, transverse-vertical and longitudinal-vertical sections of the placenta presented much the same appearance under the microscope. An examination of the fœtal margin of these placental sections showed conclusively that at no spot did the vessels pass beyond this surface of the organ, but that, on the contrary, they invariably turned upon themselves, and re-entered the placenta, and that therefore, so far as their distribution in this direction was concerned, they were clearly confined to the substance of the placenta. With regard to their arrangement at the *uterine* or papillar surface of the placenta, as already observed, I could not distinguish any clearly-defined limitary membrane bounding their distribution in this direction. At the same time, I may observe, that although very numerous sections were made with the view of determining whether these vessels ever passed beyond the placental substance, in no single instance did I ever detect such an appearance as could lead me to believe that the vessels in question had been divided, and had, previous to the detachment of the placenta, passed from the latter into the wall of the uterus. Had such a passage taken place, it seems improbable, in view of the comparatively large size of these vessels, that the torn extremities of some at least of them would not have been recognised. The absence of any such appearance lends support to the view that the vessels under consideration really constituted and belonged to the *fœtal* portion of the placenta. Horizontal sections of the placenta, that is, sections made parallel to the surfaces of the organ, presented a different appearance. In these, by far the greater number of the vessels were seen to be either obliquely or transversely divided. This showed that these transverse anastomosing vessels above described were not for the most part united at right angles with the vertical trunks with which they inosculated, but that they pursued a more or less oblique course between the latter. Had they passed off at right angles with these, the appearance of the horizontal would have closely resembled the vertical sections, whereas, whilst the latter under the microscope presented a very similar appearance, the horizontal sections differed from them in respect of the much smaller number of longitudinally divided vessels which were visible.

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[*] "Trans. Roy. Soc. Edin.," vol. xxvii, p. 79.

When a portion of the placenta was teased out with needles under the microscope, these vessels were seen to be provided with extremely thick but apparently structureless walls. When immersed in solution of logwood, the latter were readily stained by that reagent. That they were composed of a form of elastic tissue was clearly shown by the singular immunity which they exhibited to the action of different reagents. Even when boiled in dilute hydrochloric acid, or digested for eighteen hours in solution of caustic potash, the walls of the vessels underwent apparently little change, and subsequently manifested their elastic nature by the resistance which they offered to the pressure of the covering glass. Professor Turner* observes that the walls of vessels, which, in other respects, present so close a resemblance to those under consideration, which he met with in the maternal portion of the placenta of *Cholœpus Hoffmanni*, possessed "no elastic tissue or muscular fibre cells." So far as the absence of muscular fibres is concerned, the placental vessels of *Procyon* agree with those of *Cholœpus*, but differ inasmuch as the wall, although apparently homogeneous under the microscope, is distinctly elastic in character. These vessels, in *Procyon*, were provided with a distinct endothelial lining which, although not always distinguishable, was in some vessels clearly enough defined. It consisted of closely applied, flattened endothelial scales, which at some places were of an oval form, whilst at others they were fusiform in character. As seen in transverse sections, the vessels, doubtless because of their strongly elastic walls, were observed to be uniformly open. They were in some cases completely, and in others partially filled by a granular looking material, which doubtless consisted of altered blood-corpuscles. In the latter case the vascular contents had shrunk away from the wall of the vessel, leaving a clear space between them and the endothelial lining of the tube. The vessels were imbedded in a matrix of faintly fibrillated connective tissue, which, however, was small in quantity, and in some places appeared to be altogether wanting. At these places, the vessels lay directly in contact with one another.

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* "Trans. Roy. Soc. Edin.," vol. xxvii, p. 80.

It is worthy of remark, that the colossal capillaries just described constituted by far the larger mass of the placenta, and that in this portion of the organ there was an almost complete absence of capillaries of the usual description, these being confined to the circum-papillate ridges, and their prolongations, which I regard as constituting the *maternal* portion of the organ.

Having now described the histological appearances of what I regard as the *fœtal* portion of the placenta, I pass to the consideration of that of the *maternal* part. I have already shown that each of the papillæ met with on the uterine surface of the placenta was surrounded by a well-defined wall of tissue which bounded a space, in the centre of which the papilla was situated. The papillæ consisted exclusively of a mass of colossal capillaries, together with a small amount of inter-vascular connective tissue matrix. An examination of the circum-papillate ridges with a simple lens showed that these were merely the free edges of a number of elongated processes which dipped into the substance of the placenta. With a little care these ridges, together with their intra-placental prolongations, could readily be drawn out from the substance of the placenta and submitted to microscopic examination. After being teased out and placed under the microscope with a magnifying power of 440 diameters, these processes were seen to present exactly the same structure as the mucous membrane of the impregnated horn of the uterus, of which, without doubt, they were prolongations. They consisted of a slightly fibrillar connective tissue largely intermingled with clear granular cells resembling leucocytes, together with numerous spindle-shaped connective tissue corpuscles. Moreover, in certain preparations which I was fortunate enough to procure by simply drawing out the processes in question from the placental substance, I was able to observe that the surfaces of the latter were covered with a layer of columnar epithelial cells, which, at some parts, formed a continuous investment to the processes; whilst, at others, the epithelium having, to some extent, been abraded, although a continuous covering was absent, these cells, isolated, or in groups of various sizes, still adhered to the surface of the process under examination. These cells closely resembled those lining the alimentary canal, and were provided with a well-defined nucleus of a round or oval form. I have little doubt that in the recent state these cells had formed a *complete* investment to the maternal processes. It was particularly remarked that in none of these processes was there any trace of the colossal capillaries above described, but that their blood-vessels consisted exclusively of capillaries of the ordinary description, and exactly resembled those met with in the uterine mucous membrane.

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* "Trans. Roy. Soc. Edin.," vol. xxvii, p. 71.

Professor Turner has already described, in the placenta of *Cholœpus*, two sets of vessels which almost exactly correspond to those above referred to *Procyon*. His observations, however, were rendered more perfect than my own by the fact that he was able to inject the placenta both from the fœtal and maternal vessels. From the completeness of this injection he was enabled to decide that the *colossal* capillary system constituted the *maternal* portion of the placenta, whilst the capillaries which presented the usual structure appertained to the *fœtal* portion. I have, consequently, endeavoured to reconcile my own observations with those of Professor Turner, but in vain. Of a direct connexion between the *colossal* capillary system and the fœtal umbilical vessels in *Procyon*, I was, of course, in the absence of injection, unable to convince myself. But, bearing in mind the facts (1) that in no single instance did I observe any appearance of the passage of a colossal capillary beyond the placenta into the wall of the uterus; and (2) the exact correspondence in structure of the circum-papillate ridges with that of the uterine mucous membrane; and (3) the probable continuity of these processes with the mucous membrane of the uterus; I can (subject, however, to future correction) see no escape from the conclusion that in *Procyon* the colossal capillaries belong to the *fœtal*, whilst the ordinary capillaries constitute the vascular portion of the *maternal* placenta.

Having now described the minute anatomy of the placenta, I pass to that of the uterine mucous membrane. Having made sections of the wall of the left or unimpregnated uterine horn, and subjected these to the staining action of logwood, with the aid of the microscope the presence of numerous uterine glands could be readily recognised. These glands were very unequally distributed in the mucous membrane. At some places they appeared to form patches, and were so closely packed that the interval separating any two of them did not exceed half the diameter of a gland tube, whilst at others the individual glands were separated by a much larger interval. They occurred indifferently upon the transverse mucous rugæ and upon the intermediate depressions. The glands themselves corresponded in length to the thickness of the mucous membrane, their blind dilated ends closely approaching but never actually penetrating the sub-mucous coat, whilst their free extremities opened upon the surface of the mucous membrane. The gland tubes were extremely tortuous, so much so that, in some instances, their regularly spiral course reminded one of the epidermic segment of the duct of a sudoriparous gland. In some cases, the tubes terminated in a single dilated and somewhat acinous extremity, whilst in others two or even three of these dilated acini could be seen to terminate in a common tube. The character of the epithelium lining these glands could not, unfortunately, be made out, but it seems probable that it does not differ essentially from that met with in the uterine glands of other mammals. The epithelium covering the surface of the mucous membrane of the unimpregnated horn had entirely disappeared, and, therefore, its character could not be determined.

The mucous membrane of the impregnated horn was quite entire. It differed from that of the barren horn in the absence of the well-defined transverse rugæ already described. The mucous lining of this horn was much thinner than that of the opposite side, and appeared as if it had been mechanically stretched by the growth of the contained fœtus. When a portion was placed under the microscope, it was seen to differ from that of the barren horn in respect of its much softer and more succulent character. This was due to the presence of large numbers of leucocytes, as well as of spindle-shaped connective tissue cells in various stages of growth. In the barren horn, the latter were present as formed connective tissue, and, consequently, lent to the mucous membrane a denser, tougher, and more fibrous consistence. In the mucous membrane of the non-placental area of the gravid horn uterine glands were but rarely distinguishable and then only in a fragmental form. Here and there minute fragments of gland tubes were observed, but, in no case, did I recognise a *perfect* gland similar to those met with in the opposite horn. In the mucous membrane of the placental site not a trace of a uterine gland could be recognised, the glands in this area having apparently atrophied and entirely disappeared. As regards the epithelial lining of the gravid horn, no certain information could be obtained, as the mucous membrane had been almost entirely denuded of epithelial cells. At the same time, the appearance of an occasional epithelial cell, columnar in character, and attached to the uterine surface of the placenta, renders it probable that previous to detachment of that organ the uterine mucous membrane had been provided with a layer of columnar epithelial cells, similar to that observed by numerous authors in other species of mammals. The fact, moreover, that the intra-placental prolongations of maternal mucous membrane were invested by such a layer of cells lends additional support to this opinion.

I have already remarked that, with the exception of its epithelial layer, the mucous membrane of the gravid horn of the uterus, subsequent to detachment of the placenta, was quite entire, and that the uterine surface of the placenta was not invested by a complete layer of decidua serotina, such as is met with in the shed placenta of the cat.* In *Procyon*, therefore, the intra-placental prolongations of maternal mucous membrane, together with the epithelium, are alone deciduous, the mucous membrane of the uterus remaining adherent after detachment of the placenta.

Comparison of the Placentation of Procyon with that of other Carnivores.

From the particulars above detailed, it will be seen that *Procyon* agrees with the other carnivorous mammals, with the placentation of which we are acquainted, in respect of the annular or zonary form of its placenta. This form of placenta, and this alone, has been met with in the various members of the group Carnivora, the placentation of which has hitherto been examined. In the majority of these the placenta forms a complete zone, and presents no trace of the placental gap which, in *Procyon*, occurs at that part of the placental band which lies in relation to the dorsum of the embryo. The gap in question has, however, been observed in the placentæ of a few carnivores. So far as recorded observations go, while failing to recognise the presence of this gap in the placenta of any *digitigrade*, or pinniped Carnivore, they have affirmed its presence in the placenta of every member of the *plantigrade* section, the placentation of which has been hitherto examined. The latter group includes the genera *Martes*, *Mustela*, and *Lutra*, in the members of each of which a placental gap has been observed. With these, therefore, *Procyon* agrees more closely as regards its placentation than with any member of either the digitigrade or pinniped groups, in the placenta of none of which does the gap in question occur. If Bischoff's explanation of the formation of this gap be correct, it would appear that, in the plantigrade Carnivora, the closure of the true, and the consequent separation of this from the false amnion takes place at a relatively later date in the latter than in either the digitigrade or pinniped Carnivora.

But, whilst the placenta of *Procyon* differs, in this respect, from that of the members of the digitigrade and pinniped groups of Carnivora, it agrees with them in its truly deciduous character, as exemplified in the case of the bitch, cat, fox, and seal. The interlocking of the intra-placental processes of maternal mucous membrane with the fœtal chorionic villi, and the separation of both from the uterine wall, have been already described; and although the processes of maternal tissue could without difficulty be withdrawn from the interstices of the chorionic villi, it must be borne in mind that, at the time of separation of the placenta, the full period of gestation had not been completed, and that, in all probability, had such been the case the union between the fœtal and maternal portions of the placenta would have been more intimate than in the specimen examined. Although, owing to the absence of injection, the precise form of the chorionic villi and the mode of interlocking of these with the maternal processes could not be ascertained, there seems little reason to doubt that their arrangement presented a close resemblance to that described by Professor Turner in the placenta of the bitch, cat, and seal. The structure of the maternal portion of the placenta of *Procyon*, moreover, agrees in all respects with that of the animals just named, consisting as it does of processes of the mucous membrane of the uterus which are richly supplied with capillaries presenting the same structure as those of the uterine mucous membrane itself. A farther resemblance between the maternal processes of the placenta of *Procyon* and those of the other mammals mentioned is to be found in the presence, in all, of a well-defined layer of columnar epithelial cells investing these processes.

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* Eschricht, "De Organis," pp. 14, 18; and Turner, "Lectures on the Comparative Anatomy of the Placenta," p. 80.

With regard to the appearance presented by the uterine surface of the placenta, *Procyon* agrees rather with *Canis* than with *Felis*. The very regular arrangement of the uterine extremities of the fœtal villi, surrounded as they are by the circum-papillate ridges of maternal tissue, is certainly peculiar to the racoon. At the same time, an examination of the uterine surface of the placenta of the bitch, at nearly full time, convinced me that the difference in this respect between the placenta of *Procyon* and that of *Canis* is one of degree rather than of kind. In the latter, although the arrangement of these two constituents on the uterine surface of the placenta is by no means so regular as that met with in the racoon, yet indications of it were plainly observable. In the bitch, as observed by Turner,* and this observation I can confirm, the chorionic villi reach the uterine surface of the placenta much as in *Procyon*. Surrounding them are numerous ridges of maternal tissue which, although by no means presenting the regular honeycomb appearance met with in the racoon, nevertheless, at places, closely resemble it. In the bitch, these ridges for the most part enclose irregular polygonal spaces, in the centre of each of which lies the uterine extremity of a fœtal villus, whereas in *Procyon* these spaces present a regular honeycomb appearance.

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* "Lectures on the Comparative Anatomy of the Placenta," p. 82.

But farther, a comparison of the placenta of the racoon with that of the bitch and cat shows that in one important particular *Procyon* agrees with *Canis* rather than with *Felis*. As has long been known, the uterine surface of the placenta of the cat, after being detached from the uterus, is covered with a distinct and continuous layer of decidua serotina, whereas, in the bitch, as shown by Professor Rolleston,* this is not the case. In this animal, the deciduous portion of maternal tissue consists simply of the intra-placental processes which are detached along with the placenta, whilst the uterine mucous membrane is not deciduous, but remains adherent to the wall of the womb. Such is also the case in *Procyon*, and consequently in it, as in the bitch (and apparently also in the fox),† the uterine mucous membrane forms a continuous lining to the wall of the uterus, after detachment of the placenta; whereas, in the cat, according to Turner,‡ subsequent to the separation of that organ, only the deeper part of the sub-epithelial connective tissue of the uterine mucous membrane remains adherent. Whether, in *Procyon*, the epithelium of the uterine mucous membrane is normally detached along with the placenta, unfortunately I could not determine.

Professor Turner has observed that in the mucous membrane of the placental area of the sloths§ and seals,|| the uterine glands appear to undergo a retrograde metamorphosis, and to become of little functional importance during the latter period of pregnancy. The same remark holds good of the uterine mucous membrane of *Procyon* in which, as already shown, the uterine glands, although well developed, and present in large numbers in the unimpregnated horn, are with difficulty recognisable in the non-placental area of the gravid horn, and then only in a fragmental form, whilst, in the placental area, they have entirely disappeared. It would appear, therefore, that, at least in the Carnivora, the uterine glands are functionally inactive subsequent to the period when the placenta has been fully developed.

Whilst, however, the placenta of racoon agrees thus far with that of other Carnivora, it differs materially in respect of the structure of the blood-vessels which constitute by far the larger portion of that organ. As already observed, Professor Turner met with similar vessels in the placenta of *Cholœpus*. In that animal, however, the placenta is dome or bell-shaped and not annular in character. So far as I can ascertain, *Cholœpus* and *Procyon* are the only genera in which these vessels have been hitherto discovered. In every particular, except the elastic nature of their walls, the fœtal vessels of *Procyon* agree with the

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* "Trans. Zool. Soc.," v, 1863.

† Turner, "Lectures on the Comparative Anatomy of the Placenta," p. 85.

‡ *Ibid.*, p. 80.

§ "Trans. Roy. Soc. Edin.," vol. xxvii, p. 100.

|| *Ibid.*, xxvii, p. 300.

maternal vessels of *Cholœpus*. I have already referred to the difficulty of reconciling two sets of observations, in accordance with one of which, these vessels appertain exclusively to the fœtus (*Procyon*), whilst according to the other they belong as exclusively to the mother (*Cholœpus*). This difficulty must remain until the placenta of *Procyon* has been re-examined after successful injection of both the fœtal and maternal vessels. But, even granting that future research may allot the colossal capillary system of *Procyon* to the mother and the ordinary capillary system of its placenta to the fœtus, the mere presence of vessels of so peculiar and identical character in the placentæ of *Procyon lotor* and *Cholœpus Hoffmanni*—animals between which there is not a single additional point of structural resemblance,—and the absence of such from the placenta of every other Carnivore which has hitherto been accurately examined, appears to me to afford only one additional proof to the many hitherto adduced of the futility of any attempt to base a sound classification of the Mammalia upon the form and structure of the placenta. There does not, indeed, appear to be any reason why the placenta, either in respect of form, structure, or both, should agree in animals which are closely allied, or differ in species, which upon other grounds we regard as widely separated from one another. The researches of Turner and others have made us aware that, reduced to its essentials, the connexion between the mother and the child is the same in all mammals, that is, it consists of an intimate contact of the fœtal and maternal blood-vessels. This, however, is simply a physiological necessity, the accomplishment of which may be brought about in many different ways. *A priori*, therefore, the particular arrangement by means of which this is accomplished is hardly likely to be constant in, or confined to one group of mammals, or such as will result in the formation of an organ which structurally, or morphologically, is likely to be of any value in affording a basis of classification. The zonary form of placenta is met with in such widely separated groups as the Carnivora, Hyracoidea, and Proboscidea, whilst the diffused placenta is common to the Suidæ, Delphinidæ, and Equidæ, and any classification based upon the possession by forms (in other respects so widely separate) of a single organ, the placenta, simply because it possesses the same form in all, must necessarily lead to error in classification, if by classification we understand the grouping together of animals in accordance with their affinities, that is, in accordance with their entire structural organisation. Similarly, to attempt to classify the Mammalia in accordance with the structure of the placenta would lead to the association of forms which in other respects have nothing in common. The Suidæ and Lemurina, in respect of the structure and arrangement of the vessels composing this organ closely resemble one another, as do also *Procyon* and *Cholœpus* in the possession of vessels which present structural characteristics not hitherto met with in the placenta of any other mammal. And yet, to associate the members of either of these groups would, taking into consideration the wide divergence which they otherwise structurally possess, be as rational as to group together the elephant and the spotted hyæna, simply on account of the possession by the female of both of these animals of a urogenital canal, presenting peculiar features not met with in that of any other mammal. A natural classification cannot possibly be based upon the form or structure of any one organ in any group of animals, but must take into consideration the various modifications of every organ of the animal body; and more especially of those proved by research to be of great morphological, although not necessarily of equal physiological, significance. As additional evidence of the slight value to be attached to the form or structure of temporary uterine and fœtal structures as affording a basis of classification, I may refer to the fact that whilst the structure of *Procyon* is such that it has very properly led naturalists to associate that genus with the arctoid group of Carnivores, yet the fœtus of *Procyon lotor* is provided with a supernumerary cuticular investment, or epitrichium, of which, according to the observations of Welcker,* the fœtus of *Ursus* presents not a trace. Of the structure of the bear's placenta we at present know nothing, but when it comes to be examined, in view of the inconstancy of fœtal structures in the members of one and the same group of mammals, there seems to be little ground to anticipate that vessels presenting the peculiarities above described in the placenta of *Procyon*, will be found to be any more characteristic of the arctoid group, as a whole, than the epitrichium has proved to be.

The fœtal membranes of *Procyon* agree for the most part with those of other Carnivora. The absence, however, in the former of an umbilical vesicle in the advanced condition of the embryo, is remarkable, and appears to separate *Procyon* from every other member of the carnivorous group of mammals. The chorion differs in no respect from that of other Carnivora. In *Procyon*, as in *Canis*, *Felis*, and *Phoca*, the placental vessels extend beyond the placental into the non-placental area of this membrane. The allantois presents a similar arrangement in all Carnivora. With regard to the amnion, *Procyon* agrees with the members of the plantigrade section of Carnivora at the same time that it differs from those of the digitigrade and pinniped sections in the late closure of the amniotic sac, in the close attachment of that sac at one spot to the fœtal surface of the placenta, and (adopting Bischoff's view)† in the consequent deficiency of placental substance at that spot.

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* "Abhand. der Naturf. Gesellsch.," Halle, Band ix, p. 39.

† "Sitzungsber. der Akad. Wissensch.," München, 1865, p. 219.

In conclusion, it may be as well to summarise the observations contained in the foregoing pages. These show:—

1. That *Procyon agrees* with *all* the other Carnivora in which that organ has been hitherto examined in the possession of a *zonary* or annular placenta.

2. That similarly *Procyon* agrees with all in the mode of interlocking of the fœtal and maternal portions of the placenta, and in the consequent deciduate character of that organ.

3. That *Procyon* agrees with the members of the *plantigrade* section, at the same time that it differs from those composing both the *digitigrade* and *pinniped* sections of the Carnivora, inasmuch as at one spot the placenta presents a gap or deficiency, at which spot the placental structure is imperfect.

4. That *Procyon* agrees with *Canis* at the same time that it differs from *Felis*, in the absence of a continuous layer of *decidua serotina* from the uterine surface of the detached placenta.

5. That *Procyon* differs from every other Carnivore, the placenta of which has been minutely examined, in the possession of placental vessels possessed of a structure hitherto only met with in the placenta of *Cholœpus Hoffmanni* among the Edentates.

6. That *Procyon differs* from every other Carnivore, the fœtus of which has been hitherto examined, in the non-possession of an umbilical vesicle.

7. That *Procyon differs* from all Carnivora of which the young have been hitherto examined, inasmuch as the fœtus is provided with a supernumerary cuticle or *epitrichium*, a structure which has only been met with in the young of certain members of other mammalian groups.

EXPLANATION OF PLATES.

I have to acknowledge, with thanks, the kindness of my friend, Mr. Alfred Young, in drawing the accompanying excellent illustrations of the microscopic anatomy of the placenta and uterine mucous membrane.

PLATE 3.

Figure 1. Exterior of the female organs of *Procyon lotor* in the gravid state, two-thirds natural size. R.C. Right cornu uteri in which the fœtus was lodged. L.C. Left cornu uteri. O.O. Ovary encircled by the Fallopian tube. L.D. L.D. Ligamentum diaphragmaticum uteri. L.T. L.T. Ligamentum teres uteri. L.L. Ligamentum latum uteri. B. Bladder.

Figure 2. The fœtal membranes after removal from the uterus. As stated in the text, the membranes were ruptured when the specimen came into my hands. Their appearance, after I had divided the placental belt and removed the fœtus, is faithfully reproduced in this figure. A. Amnion. C.C.C. Chorion. P P. Placenta. The umbilical vessels are seen ramifying on the fœtal surface of the latter.

Figure 3 The placenta, showing the central gap which corresponds to the attachment of the amnion, as shown in fig 2

Figure 4 The fœtus with its epitrichial investment The latter has been slit open along the ventral surface of the animal

PLATE 4

Figure 5 Interior of the gravid uterus of *Procyon lotor*, two thirds natural size In the right cornu uteri the fœtus is shown enveloped by the chorionic membrane and surrounded by an annular or zonary placenta The left cornu uteri is opened to show the transversely arranged mucous folds Between the cornua is the septum uteri O U Os uteri, provided with a valve like fold of mucous membrane, which separates *the uterus from* V *the vagina* U G C Urogenital canal laid open A bristle is passed into the urethra Other letters as in fig I, Plate 3

Figure 6. Left cornu uteri natural size, seen from above The drawing shows the abdominal opening of the Fallopian tube and the relations of that tube to the ovary O Ovary C U Cornu uteri L.D Ligamentum diaphragmatis cum uteri F T Fallopian tube

Figure 7 Surface view of portion of the mucous membrane from the placental site of the gravid horn, slightly magnified

Figure 8 Section of wall of barren horn of uterus, to show the uterine glands Magnified 55 diameters

PLATE 5

Figure 9 View of uterine surface of the detached placenta, showing the honeycomb appearance, slightly magnified

Figure 10. Horizontal section (*i e*, the line of section was parallel to the surface of the organ) of the placenta, showing the colossal capillaries, for the most part divided *transversely*, magnified 235 diameters

Figure 11 Portion of the placental substance, teased out after being submitted to the action of dilute sulphuric acid The figure shows the structure of the colossal capillaries, their endothelial lining, and the mode of anastomosis between them, magnified 235 diameters

PLATE 6

Figure 12 Longitudinal vertical section (*i e*, the line of section was at right angles to the surface of the organ) of the placenta, showing the colossal capillaries, for the most part divided *longitudinally*, magnified 235 diameters

Figure 13 Transverse vertical section of the placenta, showing the colossal capillaries with the intervascular matrix The capillaries are for the most part divided *longitudinally*, magnified 235 diameters

Watson.

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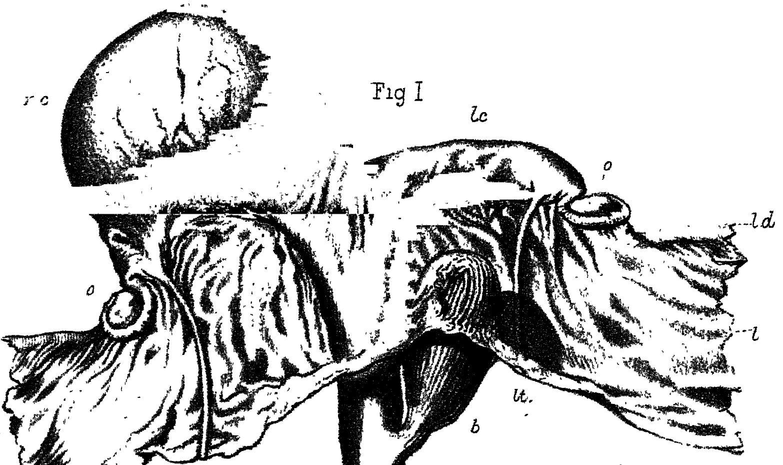

Fig I

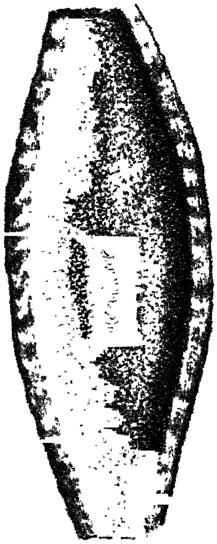

Fig 3.

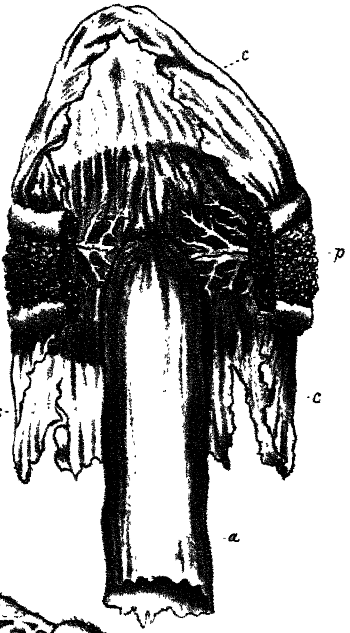

Fig. 2

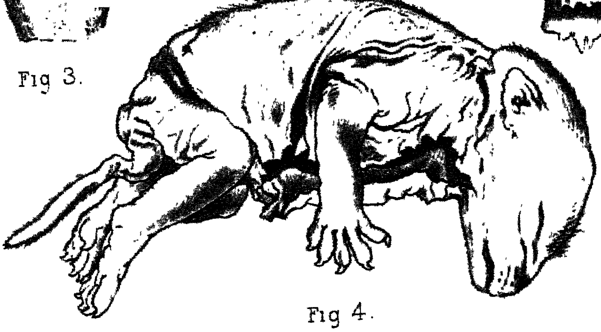

Fig 4.

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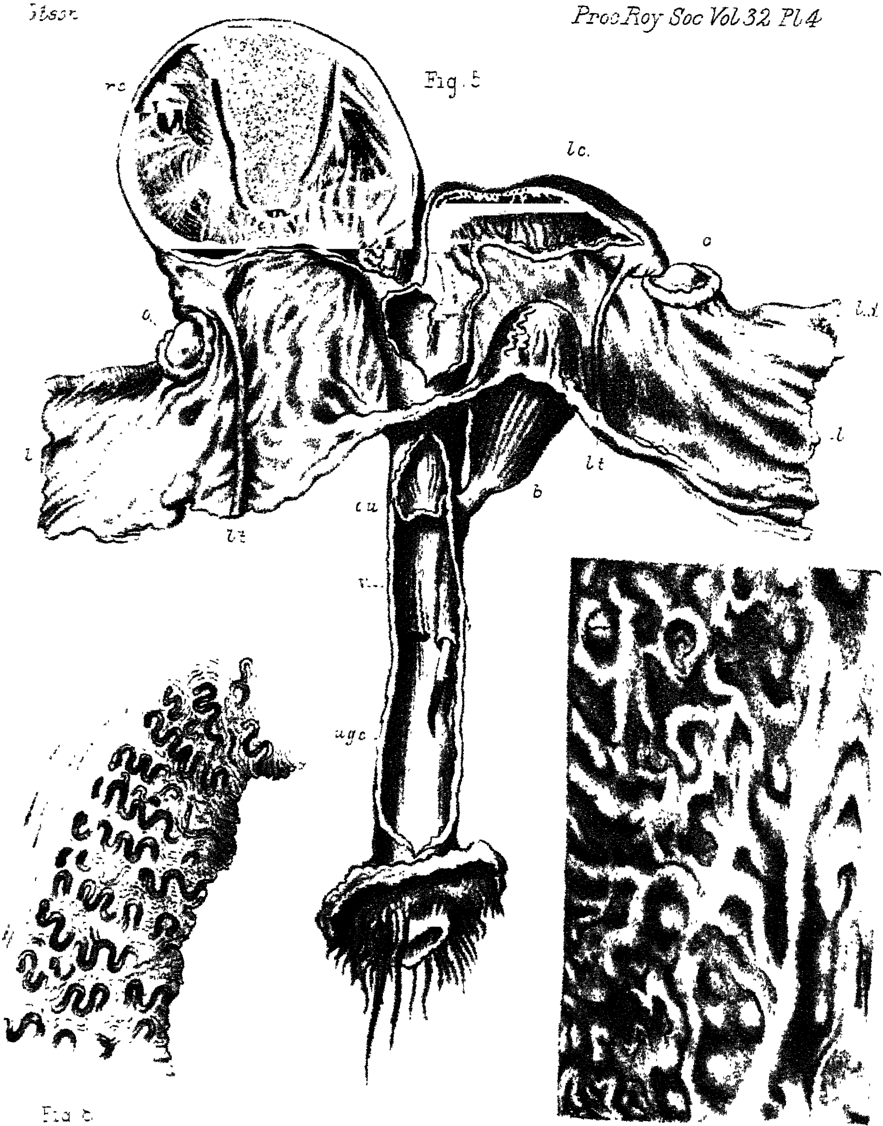

Fig. 5

Fig. 8

Fig 7.

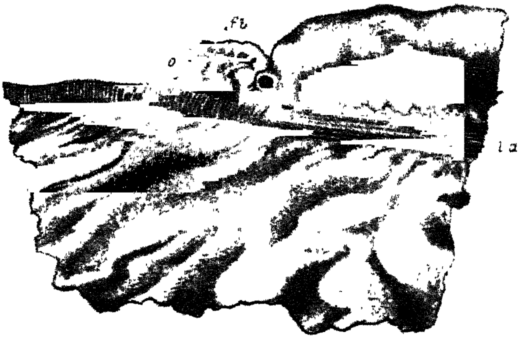

Fig 6

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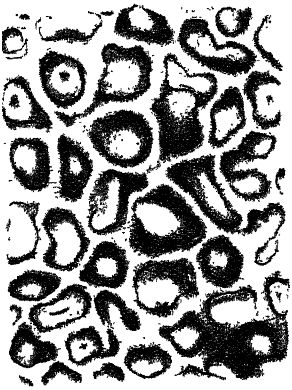
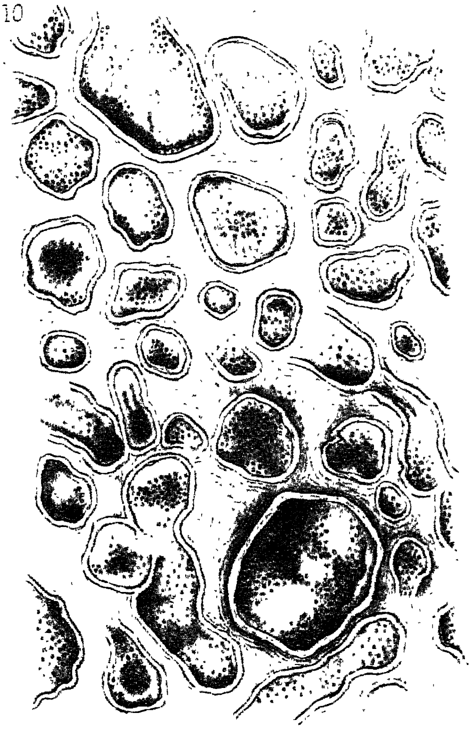


Fig 8

Fig 10



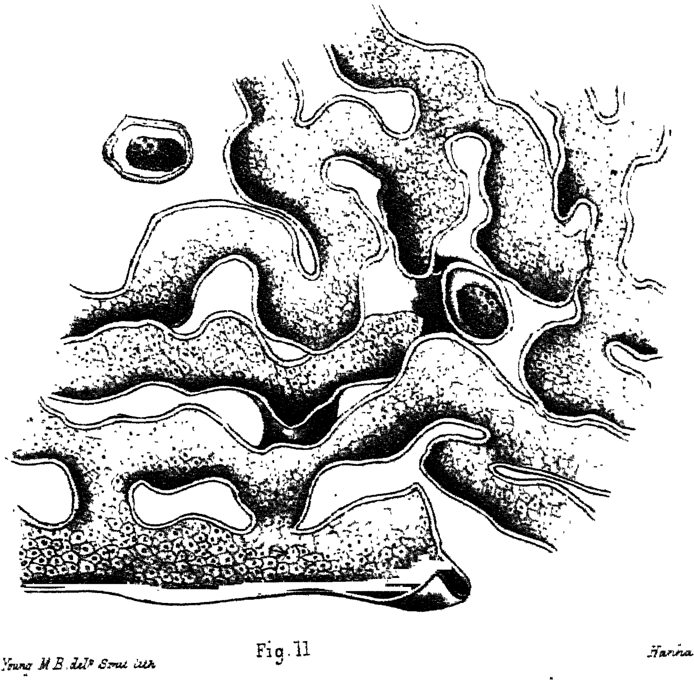


Fig. 11

A H Young M B. del^t Smit lith

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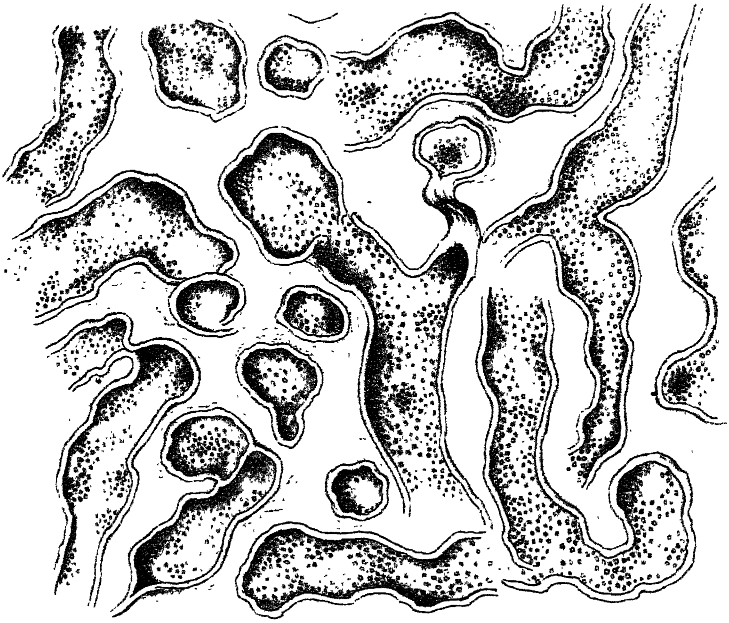
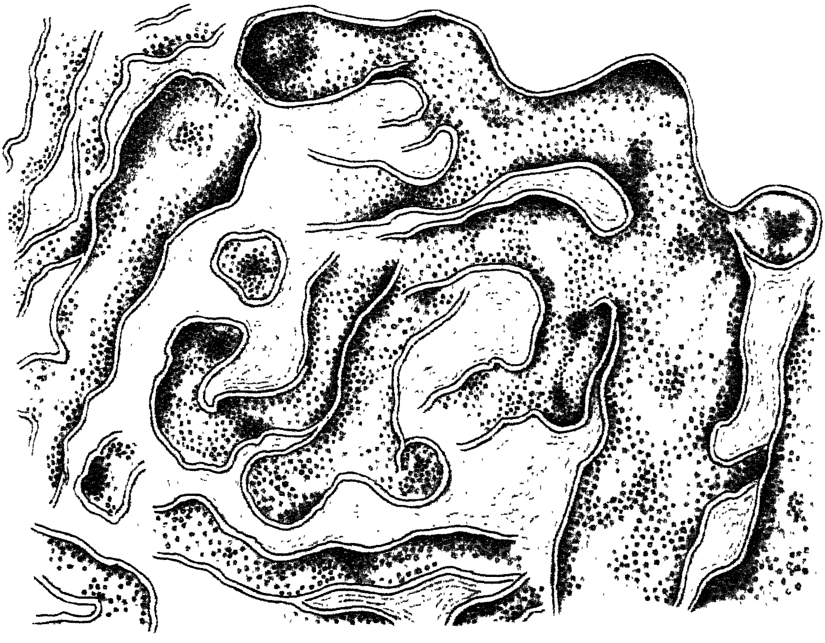


Fig 12.



A.H Young M B. del^t Smit lith

Fig 13

Hanhart imp

"On the Diastase of *Kôji*."* By R. W. Atkinson, B.Sc. (Lond.), Professor of Analytical and Applied Chemistry in the University of Tôkiô, Japan. Communicated by Professor A. W. Williamson, For. Sec. R.S. Received March 3. Read March 10, 1881.

Summary.

Section I.—*Preparation of the* Kôji.

Mechanical preparation of the rice. Addition of spores of *Eurotium oryzeæ* (Ahlb.). Growth of mycelium in warm chamber. Rise of temperature during growth sufficient to preserve the temperature of the chamber constant, and, in winter, much above the temperature of the outer air. Temperature of *kôji* itself from 10° to 23° F. above that of the chamber.

Activity of growth shown by the rapid replacement of oxygen in a confined portion of air by carbonic acid.

Loss of weight of the rice during the growth of the fungus.

Section II.—*Action of Water on* Kôji.

Amount of solid matter dissolved depended upon time and temperature of digestion, and upon the proportion of water used.

Amount of albuminoids dissolved depended mainly upon the duration of digestion.

Temperature of greatest change in cold water extract of *kôji*.

Section III.—*Action of* Kôji *Extract upon some Carbohydrates.*

Extract of *kôji* causes inversion of cane-sugar. It also effects the hydration to dextrose of maltose and dextrin. Curve A.

Extract of *kôji* breaks up the starch molecule into maltose and dextrin; the maltose is quickly hydrated, and the products after some time are dextrin and dextrose.

Experiments showing the action of *kôji* extract upon starch-paste at various temperatures from 4—10° C. to 70° C. Curves B to H.

Effect of common salt in neutralising the hydrating power of *kôji* extract.

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* I feel that some apology is needed for using the Japanese word *kôji*, but as there is no foreign product in any way resembling it, I have thought that there would be less danger of confusion arising by retaining the Japanese word than by using the word "malt." As will be seen from the following description, the nature of this substance is quite different from that of malt, so that the use of that word might lead to erroneous impressions.

The *ô* is pronounced long, as it is a contraction for the diphthong *au*, the word being written in Japanese *kauji*, but pronounced as written above.

SECTION IV.—*Change which the Rice Grain Undergoes by the Growth of the Fungus.*

It is shown by a comparison of the analyses of rice and *kôji* that the principal change which is to be observed is the conversion of the insoluble albuminoids of rice into the soluble state, and, probably as a result of this, the large increase in the total soluble solid matter.

It may be desirable before entering upon the preparation and properties of the substance which the Japanese call *kôji*, to mention briefly the uses to which it is put in this country. It is universally employed as a fermenting agent, but it is something differing from such a body as "barm or yeast," by which Dr. Hepburn translates *kôji* in his invaluable dictionary. Its principal use is in the production of *saké*, the alcoholic liquid which is everywhere consumed in Japan. This liquid is prepared from steamed rice by digestion with *kôji*, the diastase of which effects the conversion of the starch into matter capable of being fermented.

Another use to which *kôji* is applied is in bread-making. It is also employed in the manufacture of the famous sauce "Soy," which is likewise a product of fermentation, though its preparation is much more complicated, and has not yet received an explanation.

In "Nature" (September 10th, 1878), I gave a very brief account of the mode of producing *saké*, and about the same time Mr. O. Korschelt read a paper before the German Asiatic Society of Japan giving a detailed description of the process, together with some experiments upon the action of water upon *kôji*. The result of his investigation was that *kôji* acted as a kind of diastase, converting starch into sugar, but he gave no experiments which could serve to identify the product.

In a paper read before the Chemical Society in March, 1880, of which an abstract appears in the "Chemical News," April 9th, 1880, I gave a series of analyses of the mash, as the result of which the conclusion was drawn that the diastase of *kôji*, unlike that of malt, yields dextrose and dextrin when it acts upon gelatinised starch. The conclusion was correct as referring to the *ultimate* products, but further experience has shown that the first product is not dextrose, but maltose, which, however, is quickly hydrated to dextrose. Evidence of this will be found in a later part of this paper.

I.—*Preparation of the* Kôji.

The rice grain consists of several envelopes, the outer of which are easily removed in the form of chaff by threshing, and after being burnt, the mineral constituents are returned to the soil. The grain still possesses a thin adhering skin which is removed by a very rough process of beating with wooden hammers, the grain being contained in a wooden mortar.* By this operation a large proportion of the grain becomes broken, and is thus rendered unfit for many purposes, as for eating, and for *saké* brewing. In the *kôji* manufactory these broken grains, mixed with the thin skin, are converted into *kôji* in the same way as the cleaned and whole grains, but the product is inferior in colour, and is put to other uses. Besides the loss of the skin the grains are incomplete, one end of each being sharply curved inward owing to the complete removal of the embryo from the rough treatment it has received. This alone shows that the change which the rice grains undergo in being made into *kôji* cannot resemble a process of germination.

The rice thus cleaned is next soaked in water all night, and, on the following morning, it is heated in a current of steam until each grain is just soft enough to be elastic, an operation which takes from four to five hours. After this it is thrown upon mats laid upon the ground, and turned over continuously by the workmen until it is quite cool, the temperature taken on three different occasions being 84°, 86°, and 84° F. By this constant working the grains are prevented from cohering into lumps, and the mass feels to the hand quite dry.

To every two bushels the foreman adds three salt spoonfuls of the spores of a fungus, described by the late Mr. Ahlburg as *Eurotium oxyzeæ* (Ahlb.).† The whole of these spores is mixed with a small portion of the rice, and the mixture is then scattered over the rest of the rice as a husbandman scatters seed over the ground. The mats are folded over at each corner, and the rice thoroughly worked again to ensure a uniform distribution of the spores. After this the rice is gathered into baskets, and carried into the coolest part of the "growing chamber." This is done about 2 P.M. on the first day.

In the *saké* breweries, where *kôji* is made for brewing purposes, the "growing chamber" is of a somewhat rough construction, being formed near the surface of the ground, built up with timber, coated with mud, and covered with straw mats to avoid as much as possible loss of heat. In the *kôji* manufactory proper, however, it is a much more satisfactory place. It consists of a long arched passage, cut in the thick bed of clay which underlies Tôkiô at a depth below the surface of from 15 to 20 feet. In the manufactory at Yushima, in Tôkiô, with which I am best acquainted, there are in all four of these underground passages, only one of which is used during the summer, as very little *kôji* is made during that season. The height is rather less than 4 feet, the breadth about 7 or 8 feet, and from the entrance, reached by descending a vertical shaft, it extends about 25 or 30 feet in one direction, then bends off nearly at right angles for about the same distance. From the second passage two others extend, in the same direction as the first, for about 30 feet. In these the temperature of the air is highest.

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* It is a fact of some interest that if the skin be allowed to remain, the *saké* produced from such *kôji* is more liable to spoil than when the skin is removed. This is probably connected with the large percentage of albuminoids contained in the skin.

† "Mittheilungen der Deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens." 16ter Heft. December, 1878, p. 252.

Except at the beginning of the season, no fire or heating arrangement is used, but after being disused for a long time, the chamber would be too cold, and would delay too long the formation of the *kôji*.

After the rice mixed with spores has been taken to the growing chamber about 2 P.M. on the first day, it is thrown into a heap, covered with mats, and left for one night. On the following day about noon (second day) it is put into baskets, withdrawn from the chamber, and sprinkled with water. At this time the temperature of various batches which were examined varied between 75° F. and 79° F. When the *kôji* is required for *saké* brewing this operation is omitted, and the product is then called raw *kôji* (Jap. *ki-kôji*). The common *kôji*, which is prepared by sprinkling with water is, however frequently bought by the brewer for the purpose of mixing with the raw *kôji*.

About 5 P.M. on the same day (second day) the rice is spread over small wooden trays, and placed upon the floor of the growing chamber underneath the trays which at this time contain nearly finished *kôji*. The rice is spread out so that in no part is the layer of any thickness; the temperature, however, gradually rises during the night. A sample of which the temperature was taken at 8 P.M. on the second day was 80°·6 F., the temperature of the air at the same time being 81° F. At 5 A.M. on the third day the workman re-enters the chamber for the purpose of removing the trays of finished *kôji*, and of putting in their place the trays containing the mixed rice and spores of the third day. This mixture has at 5 A.M. a temperature a little higher than that of the chamber; the workman then collects the rice on each tray into a small heap, and allows it to remain undisturbed till between 9 and 10 A.M. The grains of rice on the morning of the third day present a slightly woolly appearance, which shows that the spores have already partly developed with the production of mycelial fibres. The temperature rapidly rises; at 8 A.M. the temperature of the rice is from 104° F. to 106° F., and it increases slightly until between 9 and 10 A.M., when the workman works the rice over with his hands and spreads it out to cool it. The temperature of the air during this time remains tolerably constant, as will be seen from the details of the observations given afterwards.

When the rice has become cooler it is again heaped up on the trays and left till about 1 P.M., at which time its temperature has again risen to over 100° F. After this, the heaps are broken down and the thin layers gradually become cooler, the workman frequently rubbing the matted masses between his hands to open them.

During this period the mycelium has developed very greatly, long silky fibres binding together the grains of rice. The *kôji* is now left until 5 A.M. on the following morning (fourth day), when it is found to have become cemented into one cake by the filaments of mycelium. It is then removed and preserved on the wooden trays until sold.

I have taken temperature observations at two periods in the year, in May and December. The temperature of the chamber was taken by means of a Negretti and Zambra maximum and minimum thermometer, suspended with the middle of the long cylindrical bulb about one foot above the trays containing *kôji*, and about the same distance below the roof. The temperature of the *kôji* was taken by means of a small tube thermometer plunged into the mass on the trays and allowed to remain several minutes, until no further change was observed. The temperature of the rice on several trays was taken and the mean of the observations regarded as the true temperature.

Table I.—Temperatures of Chamber and *Kôji* during May.

I. Date.	II. Hour.	III. Temperature of air outside the chamber.	IV. Minimum temperature of chamber.	V. Maximum temperature of chamber.	VI. Temperature of *kôji* on third day.
May 18 ..	8 A.M.	55·3° F.	72° F.	76° F.	No *kôji*.
„ „ ..	6 P.M.	61·8	72	74	„
„ 19 ..	7 A.M.	59·0	72	77	89·6° F.
„ „ ..	8 P.M.	64·0	74	76	
„ 20 ..	8 A.M.	58·7	76	77	84·2
„ „ ..	9 P.M.	64·6	75	77	
„ 21 ..	7 A.M.	60·5	75	76	
„ „ ..	9 P.M.	65·0	74	76	86·0
„ 22 ..	9 A.M.	63·6	75	77	86·0
„ „ ..	9 P.M.	60·0	76	79	89·8
„ 23 ..	7 A.M.	65·5	77	83	
„ „ ..	8 P.M.	65·0	79	82	95·0
„ 24 ..	7 A.M.	64·0	80	81	102·0
„ „ ..	8 P.M.	66·5	78	80	86·0

The temperature of the chamber from the 18th to the 23rd remained tolerably constant, but then for some reason an increase in both maximum and minimum temperatures took place. The temperature of the rice at the time of its introduction varied, as before mentioned, from 75° to 79° F., and was, therefore, a little lower than that of the chamber. In Column VI is given the temperature of the *kôji* on the third day. It varied considerably, sometimes rising very high, but always being above the maximum temperature of the chamber. The increase in the temperature of the *kôji* in May is by no means so marked as in the second series of observations made in December, during which month much more *kôji* is made than in May. The temperatures given below refer, like the last, to *kôji* of the third day only, as it is during that time that the greatest amount of growth takes place.

Table II.—Temperature of Chamber and *Kôji* during December.

I. Date.	II. Hour.	III. Temperature of air outside the chamber.	IV. Minimum temperature of chamber.	V. Maximum temperature of chamber.	VI. Temperature of *kôji* on third day.
Dec. 5....	8 A.M.	40·7° F.	..	..	104·8° F.
„ „....	2 P.M.	49·5	82·0° F.	83·0° F.	91·9
„ „....	8 „	42·5	81·0	83·0	88·8
„ 6....	8 A.M.	41·5	80·0	83·0	106·6
„ „....	10 „	44·7	81·6	82·0	101·0
„ „....	1 P.M.	50·0	81·0	82·5	104·1
„ 7....	9 A.M.	38·5	80·0	82·5	104·2
„ „....	2 P.M.	51·0	80·5	82·0	93·6
„ 8....	8 A.M.	37·5	79·0	82·5	100·0

If we take the average temperature on the three mornings at 8 A.M., and one at 9 A.M., we find it to be 103°·9 F., and the average for the maximum temperature at the same time is 82°·7, hence the *kôji* has at 8 A.M. an average temperature 21°·2 F. higher than the chamber. The observation at 10 A.M. on December 6 was intended to ascertain what the highest temperature attained was, which the workman said occurred about that time; but on that day the rice became so hot that he was obliged to spread it out before its temperature was taken, so that the number 101° F. gives the temperature after partial cooling. The observation at 1 P.M. was made before the rice was spread out for the second time, and it will be seen that the temperature has risen considerably. With these results, one cannot doubt the truth of the statement made by the workman that the rice becomes heated during the growth of the mycelium. Mr. O. Korschelt* has previously remarked that the temperature of the rice rises during the formation of *kôji*, but he gives only one observation. He says: "Ich fand dann die Temperatur des Reises zu 25° C., während die Luft in der Kammer nur 20° C. zeigte. Die nöthige Wärme also durch den Process selbst entwickelt."

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* "Mittheilungen der Deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens." December, 1878. 16ter Heft., p. 241.

The rise in temperature is almost certainly due to respiration, a process common to all plants. I know of no case, however, in which the increase is so great as in the one under consideration. Sachs says that in the spadix of the Aroideæ at the time of fertilisation, and especially in warm air, an excess of temperature of from 4° to 5° C., or even 10° C., has been detected.* In the present case the respiration is sufficiently active not only to keep the temperature of the rice above that of the surrounding air about 21° F., but at the same time to keep the temperature of the air in the chamber from 30° to 40° F. above that of the air outside, because no artificial source of heat is made use of.

It is a little remarkable that the variation in the temperature of the air in the chamber is so small, for, as the place is not artificially warmed, but depends upon the heat given out by the growing fungus, it might be expected to be much higher in the morning than in the evening. Such a variation will, indeed, be noticed, but not to a greater amount than 2° or 3° F. The constancy in the temperature is no doubt the result of having successive batches in the same chamber. During the morning only the actively growing *kôji* is there, and in the evening, when the former is cooling down, a new batch is brought in, which gradually rises in temperature during the night and attains its maximum between 8 and 12 in the forenoon.

That the growth of the fungus takes place with great vigour is shown not only by the rise in temperature, but by the rapidity with which it removes oxygen from the air. A specimen of the actively growing *kôji* (at 8 A.M.) was placed in a bottle of about 4 litres capacity and well corked. Tubes were arranged so that a specimen of the enclosed air could be removed from the bottle, and it was then left in the chamber from 8 till 12 noon. Over the *kôji* which was exposed to the air the mycelium of the fungus had developed considerably and had caused the grains of rice to cake together into a solid mass. The rice in the bottle, however, remained quite loose, and no increase in the amount of the mycelium could be observed. Analysis of the air in the bottle showed that the whole of the oxygen had been replaced by carbonic acid. The inference to be drawn from this experiment is that so long as the oxygen was present the mould went on growing, but that the amount of air contained in the bottle was insufficient for its growth, and hence, instead of becoming matted together, the grains of rice remained distinct.

The diminution in the weight of the rice is considerable. Mr. Jihei Kamayama was good enough to weigh for me the rice used in one operation, and the *kôji* which was formed from it. He found that 100 parts of the whitened rice, after being converted into *kôji*, weighed 101·3 parts. Samples of each were obtained and the amount of water determined. The rice contained 14·2 per cent. of water and the *kôji* 29·5 per cent; thence 85·8 parts of the dried rice yielded 76·35 parts of dry *kôji*, that is, 89 per cent., and thus a loss of weight occurs amounting to 11 per cent. of the dry rice used. This loss consists mainly of starch, which is oxidised to carbonic acid and water, and corresponds to an oxidation of 4·9 per cent. carbon. More carbon, therefore, undergoes oxidation than Day found in the case of germinating barley,* which varied from 2·3 to 2·6 per cent.

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* Sachs, "Lehrbuch der Botanik," 4th edition, 1874, p. 694.

As in one day about 107 lbs. of dry rice are converted into *kôji* in each chamber, the amount of carbonic acid formed will be $\frac{11}{3}\times 5\cdot24=$ 19·2 lbs., and such a large quantity evolved in situations from which it is difficult to remove it frequently leads to stoppage of the work for some time. The only means adopted of effecting a change of air consists of a square shaft about 8 inches in one direction and 6 inches in the other, leading from the anterior end of the passage into the open air above. It will be evident, therefore, that as the ventilation depends upon the difference of temperature between the inner and the outer air, it will be much better in winter than in summer. In fact, it is in the spring and early summer that the stoppages occur, for, as the temperature observations taken during May show, the difference at that period between the inner and outer air is not more than a few degrees. Under such conditions the growth of the fungus must be much less active, and perhaps this is one reason why the production of *kôji* in the summer is almost abandoned.

II.—*Action of Water on* Kôji.

The *kôji* prepared in the manner just described consists of grains of rice bound together in lumps by the interlacing threads of mycelium. A single grain separated from the rest presents a peculiar woolly appearance on the surface, but does not appear to have increased very much in bulk. Under the microscope, a section shows that the outer cells are loose and penetrated by the fibres of the mycelium, whilst in the centre of the grain the cells have a horny appearance, and the starch granules cannot be distinguished.

When the *kôji* grains are allowed to remain in contact with water for some time, a large proportion is dissolved by the water, which then assumes a yellow colour. Very little of the *kôji* remains except the mycelial fibres and the skeleton of cellulose when the digestion is made in warm water. The amount of matter brought into solution varies according to the duration and the temperature of digestion. At low temperatures, and after about fifteen or twenty minutes, from 12 to 15 per cent. of solid matter, calculated on the *kôji* used, is found in solution; but if the digestion is allowed to go on at a higher temperature, or for a longer time, from 30 to 60 per cent. will be dissolved.

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* "Journ. Chem. Soc.," Trans., 1880, pp. 651-657.

In Table III some analyses of the solution of *kôji*, made at the ordinary temperature, are given. They are taken from a large number made, and are selected to show the influence of varying proportions of water used, and also that of length of digestion.*

Table III.—Composition of the Solution made from 100 grms. of *Kôji* at 10° to 15° C.

No.	Total volume of solution of 100 grms. of *kôji*.	Time of digestion.	Total solid matter per cent.	Average percentage of solids.	Glucose per cent. of total solid matter.	Specific rotatory power ρ.	Average specific rotatory power.
	c.c.	hrs.					
1	500	12	17·7	..	60°·0	65°	
2	1,000	18	25·7	27·0	..	61	57°·6
3	,,	,,	24·2		..	55·7	
4	,,	,,	23·0		..	56·0	
5	,,	12	33·3		49·0	65·3	64·6
6	,,	,,	33·3		51·0	65·4	
7	,,	,,	29·4		45·0	62·9	
8	,,	,,	28·6		46·5	67·7	
9	,,	,,	26·8		53·0	61·4	
10	,,	,,	22·5		53·0	64·5	
11	,,	,,	22·2		54·0	65·0	
12	,,	4	28·0		..	61·4	
13	2,000	3	31·1	..	68·0	78·0	
14	2,500	,,	32·2	31·4	58·0	68·1	69·3
15	,,	,,	32·5		70·0	65·2	
16	,,	,,	30·7		65·0	73·8	
17	,,	,,	30·1		68·0	70·2	
18	5,000	24	30·0	..	47·0	64·5	
19	10,000	,,	40·0	..	66·0	60·5.	

These examples will suffice to show how variable the amount of solid matter dissolved by the water is, even under apparently the same conditions. Thus when 100 grms. were dissolved in 1,000 cub. centims. of water, and kept for twelve hours at a temperature of 10° to 15° C., the amount of solid matter dissolved varied from 22 per

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* The analysis of the solutions of *kôji* was carried out in the following manner. The total solid matter was calculated from the specific gravity of the solution taken at 16° C., compared with water at the same temperature, by dividing the excess above 1,000 (water = 1,000) by 3·86. This gives the number of grms. in 100 cub. centims. of liquid, from which the percentage is easily found. The glucose was determined by the gravimetric process recommended by O'Sullivan ("Journ. Chem. Soc.," 1876, [ii], p. 131).

The specific rotatory power of the solid matter in solution was determined by observing the number of divisions upon the scale of a Soleil-Duboscq saccharometer which corresponded to uniformity of tint of the two halves of the field. The light was always observed through a column of liquid 200 millims. long. The specific cent. to 33·3 per cent. The percentage of glucose contained in the solid matter varies from 45 to 68 per cent., but usually ranges between 50 and 60 per cent. The specific rotatory power of the solution made in the cold does not vary much outside the limits 60° and 70°.

The solution contains dextrose, dextrin, and albuminoids, the two latter in nearly equal amounts. In some solutions in which the nitrogen dissolved was determined, the specific rotatory power of the albuminoids was found to be −40°, and it will, probably, not be far from the truth to take this number as the average specific rotatory power of the albuminoids.

Two points are brought out by a consideration of Table III. In the first place the proportion of water used appears to influence considerably the amount of solid matter brought into solution. The average of the numbers obtained when 100 grms. of *kōji* were dissolved in 1,000 cub. centims. is only 27, whilst when the same weight was dissolved in 2,500 cub. centims. the average was 31·4. The single experiments given also bear out this with the exception of No. 18; but in a matter of this nature no reliance can be placed upon isolated observations. The second point is that the specific rotatory power of the solids in solution appears to be mainly influenced by the length of the digestion, being much higher when the digestion is short than when it is continued for some time. The average specific rotatory power of the solutions made in eighteen hours was 57°·6; after twelve hours, 64°·6; and after three hours, 69°·3; although the last number is not fairly comparable with the two former ones, a different proportion of water having been used. The explanation of this fact is probably that when the digestion is continued for only a short time, a smaller proportion of albuminoids enters into solution than when the digestion is long continued. It is probable, in fact, that the albuminoids require time to be degraded to such a form that they are soluble in water. Hence, when the amount of albuminoids is small, the dextrose and dextrine mainly contribute to the specific rotatory power, which is, therefore, higher.

In Table IV are given a few results of experiments in which the mixture of *kōji* and water was heated for different periods.

rotatory power (which I have expressed throughout the paper by the symbol ρ) was calculated from the formula—

$$\rho_1 = \frac{n \times 0{\cdot}242}{2 \times \epsilon}$$

where n is the number of divisions read off from the scale,

0·242 is the factor for converting scale-divisions into degrees of arc (cane-sugar being taken =73°·8),

ϵ is the amount of solid matter in 1 cub. centim. of solution in grams.

Table IV.—Composition of the Solution made from 100 grms. of *Kôji* at Higher Temperatures.

No.	Time and temperature of digestion.	Total volume of solution of 100 grms. of *kôji.*	Total solid matter per cent. of *kôji* used.	Dextrose per cent. of total solid matter.	Specific rotatory power ρ.
		cub. centims.			
1	2 hours at 50° C. + 18 hours at 15° C.......	1,700	51·8	68·0	68°
2	½ hour at 45° C........	2,000	31·8	84·9	76·1
3	2 hours at 45° C.......	2,000	61·6	68·5	53·5
4	½ hour at 50° C........	5,000	37·2	66·0	68·2
5	24 hours at 15° C. + 2 hours at 100° C......	10,000	49·2	58·0	73·8

The percentage of solid matter dissolved out is greater in most of these experiments than in those conducted at the ordinary temperature. Experiments 2 and 3 (Table IV) are interesting as they were made at the same time, with the same specimen of *kôji*, and with the same proportion of water, the only difference being in the time of digestion. It will be useful to compare them with the results of an experiment made with the same *kôji* at the ordinary temperature of the air. 100 grms. of the *kôji* were digested with the 2,000 cub. centims. of water at 10° to 12° C. for eighteen hours. The results of the three experiments are grouped together in Table V.

Table V.

	Total solid matter per cent. of *kôji.*	Dextrose per cent. of *kôji.*	Dextrose per cent. of total solid matter.	Specific rotatory power ρ.
18 hours at 10—12° C....	29·2	20·25	69·3	66°·3
½ hour at 45° C..........	31·8	27·00	84·9	76·1
2 hours at 45° C.........	61·6	42·20	68·5	53·5

There is not much difference in the amount of solid matter dissolved out in the two first experiments, but the proportion of dextrose contained in the second is very much higher than in the first. In the third, the total solid matter has increased very greatly, but the dextrose dissolved has not kept pace with it, and forms only the same proportion as in the solution made in the cold. The lower specific rotatory power shows, however, that the proportion of the other constituents has not remained the same. This will be better seen by calculating the amount of dextrine, albuminoids, and dextrose from the specific rotatory power, assuming that of the albuminoids to be —40°, an assumption which, if not quite accurate, will, however, not materially influence the results.

Table VI.—Composition of the Solid Matter in Solution.*

	Dextrose per cent.	Dextrin per cent.	Albuminoids per cent.
18 hours at 10—12° C.	69 ·3	14 ·7	16 ·0
½ hour at 45° C.	84 ·9	12 ·5	2 ·6
2 hours at 45° C.	68 ·5	10 ·0	21 ·5

It is evident that the percentage of dextrine is smaller when the digestion is carried on at a higher temperature, but that relatively to the amount of dextrose it has not altered within the time limits of the experiments at 45° C. will be best seen by calculating the amounts of dextrine and albuminoids per 100 parts of dextrose.

Table VII.—Amounts of Dextrin and Albuminoids per 100 of Dextrose.

	Dextrin.	Albuminoids.
11 hours at 10—12° C...	21 ·2	23 ·1
½ hour at 45° C.........	14 ·7	3 ·06
2 hours at 45° C.	14 ·6	25 ·3

The amount of albuminoids dissolved out after half an hour's digestion at 45° C. is only about one-eighth of the quantity dissolved after two hours. This bears out the observations made with respect to the cold solutions of *kōji*, viz., that the longer the time of digestion the greater was the amount of albuminoids dissolved. It may reasonably be inferred from this that the albuminoids, as they exist in *kōji*, are not in a soluble form, or not so to any great amount; but by the growth of the fungus the albuminoids of the rice have been brought into such a state that they are easily degraded by the action of water and rendered soluble.

The action of heat upon the cold water extract is not very marked except at temperature between 45° C. and 60° C. It then produces a slight difference in the colour of the liquid, which becomes more red; it reduces slightly the specific rotatory power, and increases the amount of glucose, owing partly to the hydration of the dextrin, and partly to the precipitation of a portion of the albuminoids, especially at the higher temperatures, when the liquid always becomes more or less turbid. The following series of experiments (Table VIII) was made by Mr. Y. Watanabe, Graduate of the University of Tôkiô, and shows very clearly the effect of heating for one hour at the specified temperatures.

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* Calculated thus :—

k = grm. of glucose in 1 grm. solid matter;
x = grm. dextrine;
$1-(x+k)$ = grm. albuminoids;
ρ = observed specific rotatory power.

$\rho = 216x + 59k - 40\{1 - x - k\}$, from which x, the unknown quantity, is found.

Table VIII.—Action of Heat upon the Cold-water Solution of *Kôji*.

Temperature.	Solid matter in 100 cub. centims. Grms.			Dextrose in 100 cub. centims. Grms.			Specific rotatory power. ρ.		
	Unheated.	Heated.	Increase.	Unheated.	Heated.	Increase.	Unheated.	Heated.	Decrease.
30° C....	4·88	..	..	2·97	3·015	0·045			
35 ...	4·88	..	..	2·97	3·062	0·092			
40 ...	4·89	..	..	2·98	3·079	0·099			
45 ...	4·92	4·98	0·06	2·92	3·412	0·492	74°	70°·0	4°
50 ...	4·95	5·02	0·07	2·79	3·285	0·495	70	67·1	2·9
55 ...	4·92	5·00	0·08	2·92	3·463	0·543	74	68·9	5·1
60 ...	4·95	5·02	0·07	2·79	3·30	0·510	70	67·8	2·2
65 ...	4·89	..	..	2·98	3·081	0·101			
70 ...	4·89	..	..	2·98	3·075	0·095			

Below 45° C. the increase in the specific gravity was so small that it could not be determined with any certainty, and it will be seen that although there is an increase in the amount of dextrose the increase is very small. Between 45° and 60° C. the increase in total solid matter and in glucose, and the diminution in the specific rotatory power, are more marked. From the columns showing the increase in solid matter and glucose, it will be seen that the greatest change occurs at 55° C.; and this is borne out by the diminution in the specific rotatory power. which is greatest at that temperature. The limits of greatest change are from 45° to 69° C., for either below or above these points there is a sudden falling off in the increase of dextrose.

III.—*Action of Solution of* Kôji *upon some Carbohydrates.*

(1.) *Action of* Kôji *Extract upon Cane-sugar.*—Extract of *kôji* has the property of causing inversion of a solution of cane-sugar, as the following experiments will show:—

1·974 grm. of dry cane-sugar was dissolved in 25 cub. centims. of *kôji* extract, then diluted to 100 cub. centims. The amount of optical rotation was found, by observation, to be 15·8 scale-divisions, and the number calculated was 15·5. Thus—

1·974 grm. cane-sugar dissolved in 100 cub. centims. would give rotation	=12·1	divisions.
25 cub. centims. of *kôji* solution diluted to 100 cub. centims.	= 3·4	,,
	15·5	,,

After being allowed to stand for eighteen hours the rotation was observed to have diminished to 5 scale-divisions, and the solution contained 1·67 grm. of glucose. Deducting 0·36 grm. glucose contained in 25 cub. centims. of *kôji* solution, the amount formed from the cane-sugar was 1·31 grm., equivalent to 1·2445 grm. cane-sugar; and hence 0·7294 grm. of unaltered cane-sugar was present. We thus find the calculated number of divisions of rotation to be +5·43, against 5 divisions actually observed.

Unaltered cane-sugar (·7294 grm.) in 100 cub. centims.	+4·4	scale divisions.
Kôji extract (25 cub. centims. in 100 cub. centims.)	+3·4	,, ,,
Invert sugar formed (1·31 grm. in 100 cub. centims.)	−2·37	
	+5·43	

If the calculation be made in degrees of arc, it will be found that the specific rotatory power of the solution had diminished from $\rho=74^\circ$ to $\rho=10^\circ$.

The progress of the inversion of the solution of cane-sugar will be seen from the following observations taken at successive intervals of time:—A solution of cane-sugar containing 5·41 grms. in 100 cub. centims., and giving in a 200 millims. tube an optical rotation of 33·1 scale-divisions (equal to $\rho=74^\circ$), was employed. 75 cub. centims. of this solution were mixed with 25 cub. centims. of a solution of *kôji* which contained 0·365 grm. of solid matter, 0·253 grm. glucose, and which gave in a 200 millims. tube an optical rotation of 8 scale-divisions. It may be remarked that from these and other experiments made with the same solution of *kôji*, it was found to be exceptionally weak in its inverting power. The observations are as follows, deduction having been made for the *kôji* solution present:—

At commencement solution gave rotation=24·8 div. ∴ $\rho=74^{\circ}{\cdot}0$
After $1\frac{1}{2}$ hour at 15° C. .... =23·7 „ ∴ $\rho=70{\cdot}6$
After $20\frac{1}{2}$ hours at 10—12° C. .... =21·0 „ ∴ $\rho=62{\cdot}6$
After $\frac{3}{4}$ hour longer at 40° C. .... =20·0 „ ∴ $\rho=60{\cdot}2$

50 cub. centims. of this mixture and 25 cub. centims. of *kôji* were heated for a further period. Corrected for *kôji* added—

After $1\frac{1}{4}$ hour at 40° C. rotation.... 11·2 divisions ∴ $\rho=50^{\circ}$
After 2 hours longer at 45—50° C. 4·0 „ ∴ $\rho=17{\cdot}8$

It will be seen that the action is very slow at low temperatures, and even as high as 40° C. it is not very rapid; but it becomes much more so between 45° and 50° C., a temperature at which the *kôji* has been found to be most active in converting starch.

(2.) *Action of* Kôji *Extract upon Maltose.*—The Japanese prepare a kind of sweetmeat by the action of malt-extract upon steamed rice or millet, and this product, called *âmé*, from the examination of a large number of specimens, was found to contain from 68 to 94 per cent. of maltose.* A quantity of maltose was prepared from this according to the directions given by Mr. O'Sullivan,† but the purest product which could be obtained contained sufficient impurity to reduce the specific rotatory power from 150° to 144°·5. For the present purpose it was unnecessary to use an absolutely pure specimen, so this was employed without continuing the attempt to purify it completely:—

100 cub. centims. of a solution of maltose containing 1·334 grm. of solid matter, and the equivalent of 0·855 grm. glucose were mixed with 100 cub. centims of *kôji* solution containing 3·572 grms. of solids and 2·14 grms. glucose, and the mixture heated for $2\frac{1}{2}$ hours to 35° to 40° C. The liquid after heating (deduction having been made for the *kôji* solution present) contained in 100 cub. centims. 1·374 grm. solid matter and 1·348 grm. glucose. It is evident, therefore, that the solution of maltose had been completely converted into glucose.

A solution of maltose was prepared containing 2·68 grms. of solid matter in 100 cub. centims., and which gave in a 200 millims. tube a rotation of 32·1 divisions, corresponding to a specific rotatory power $\rho=144^{\circ}{\cdot}5$. 100 cub. centims. of this solution were mixed with 100 cub. centims. of *kôji* extract containing 2·3 grms. of solid matter, and giving in a 200 millims. tube an optical rotation of 10·5 scale-divisions. This mixture was heated to 60° C. for $2\frac{1}{2}$ hours, then cooled and diluted to 250 cub. centims. at 15° C. It then contained 2·03 grms. of solid matter in 100 cub. centims., and gave an optical rotation of 11·5 divisions. Deducting the rotation and amount of solid matter due to the solution of *kôji*, we find as the result of the action upon the maltose—

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* "Transactions of the Asiatic Society of Japan," vol. vii, p. 313.

† "Journal of the Chemical Society," 1876, [ii], p. 128.

	Before heating.		After heating.
Total solid matter........	2·68 grms.	....	2·775 grms.
Optical rotation..........	32·1 divns.	....	7·3 divns.
Specific rotatory power, ρ.	144°·5	....	79°·6

The hydrating action of *kôji* upon maltose is, therefore, very decided. The action ceased at 79°·6, probably because the activity of the *kôji* was exhausted at that high temperature. The following series of experiments shows the progressive reduction in the specific rotatory power of the solution when observed at successive intervals of time. 100 cub. centims. of the same solution of maltose were mixed with 100 cub. centims. of a freshly prepared extract of *kôji*, which contained 2·424 grms. of solid matter in solution, and which gave an optical rotation of 11 divisions in a 200 millims. tube. The mixture of maltose and *kôji* solutions was diluted to 500 cub. centims. at 15° C., and after standing ten minutes a sample was withdrawn for analysis. The remainder was placed in a water-bath heated to 45° C., and samples were withdrawn after the lapse of thirty minutes, one hour, and two hours, by which time all the solution had been used up. The numbers given are corrected for the *kôji* solution added.

Table IX.

Time.	Total solid matter in 500 cub. centims. (corrected for *kôji*).	Optical rotation in 200 millims. tube (corrected for *kôji*).	Specific rotatory power of maltose products.
	Grms.	Divisions.	°
Original solution	2·685	6·5	144·5
After 10 minutes at 15° C. ..	2·826	5·8	124·2
„ ½ hour at 45° C.......	2·886	5·3	111·1
„ 1 „ „		4·7	98·5
„ 2 hours „		3·7	77·6

The action is very regular, as will be seen from Curve A, which expresses graphically the above numbers. There is no evidence at the end of two hours of the action of the *kôji* extract upon the maltose having been exhausted, and the fall in the specific rotatory power would doubtless have continued to 59° if the experiment had been allowed to continue longer. The fact that *kôji* solution thus converts maltose into dextrose is evidence of the difference of this diastase from that contained in malt, which, according to the experiments of Brown and Heron,* has no action upon maltose. It is a point of interest to observe that whilst cane-sugar yields invert sugar on hydration, its isomer maltose yields only dextrose, a fact which indicates a difference in the chemical constitution of the two bodies, arising probably from the union of levulose and dextrose to form an ether in the case of cane-sugar, whilst in the case of maltose the ether is formed of two molecules of dextrose.

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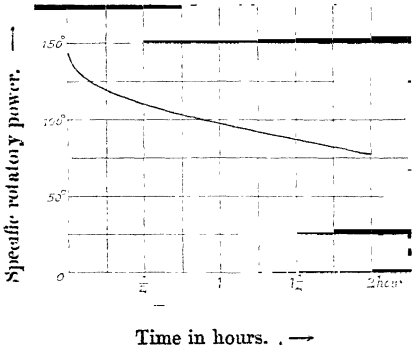
* "Journal of the Chemical Society," 1879, Trans., p. 648.

CURVE A.

Action of *kôji* extract on maltose.



(3.) *Action of* Kôji *Extract upon Dextrin.*— A specimen of commercial dextrin was used, probably prepared by the action of heat upon starch, although its history was not known. 50 cub. centims. of a solution of this dextrin containing 2·78 grms. of solid matter, and having a specific rotary power, ρ=174°, were mixed with 50 cub. centims. of solution of *kôji*, and the mixture heated for 1½ hours at a temperature of 45° C. After heating, the solution contained (deduction having been made for the *kôji* added) 2·86 grms. solid matter, and possessed a specific rotatory power, ρ=92°. The activity of the *kôji* solution was destroyed, because when an additional amount of *kôji* solution was added, and the mixture heated for a longer time, the specific rotatory power of the dextrin products further diminished to 85°.

This experiment, which has been confirmed by others, leaves no doubt that dextrin gradually becomes hydrated under the influence of the diastase of *kôji*.

(4.) *Action of* Kôji *Extract upon Gelatinised Starch.*—When extract of *kôji* is added to thick starch-paste at the ordinary temperature of the air (*i.e.*, from 15 to 20° C.), in about ten minutes the paste becomes very thin, but the solution does not become transparent. When, however, the same experiment is made at any temperature between 35° and 55° C., the paste becomes thinly liquid within a minute and a half, and is perfectly transparent in from five to ten minutes. These results are obtained with a solution containing in 500 cub. centims. the soluble matter of 25 grms. of *kôji;* but it varies with different samples to some extent, and also with the same sample at different periods, the activity becoming less as the sample grows older. If *kôji* solution of greater strength than that mentioned is employed, the hydrating action upon the maltose takes place so rapidly, that its formation may be overlooked. For a long time, I was under the impression that no maltose was formed, and it was only by using much weaker solutions of *kôji*, that satisfactory evidence could be obtained of its production.

The following experiments will, I think, show that the first action of the *kôji* extract is to split up the starch molecules into maltose and dextrin, and that when the action is continued with a greater quantity of the active agent, the maltose is completely hydrated to dextrose, whilst if samples be taken at intermediate periods, the solution will be found to contain both maltose and dextrose. In this respect, the *kôji* diastase differs markedly from malt extract, which Brown and Heron have shown to possess no action upon maltose. It resembles, however, the diastatic ferment of the pancreas, which the same observers,* confirming the work of Musculus and De Mering,† have shown to effect the hydration of the maltose first formed.

The mode of analysis followed was essentially the same as that described in detail by Brown and Heron.‡ I will give in full the results of one experiment to illustrate the method.

A *kôji* solution was prepared by digesting for a short time 25 grms. of a freshly prepared sample of *kôji* in about 100 cub. centims. of water. The liquid was then filtered, the residue digested with a fresh quantity of water, and the whole thrown upon the filter and washed, until the filtrate amounted nearly to 500 cub. centims. The solution was then diluted exactly to 500 cub. centims. at 16° C. The filtration occupied three or four hours, even with the assistance of a filter-pump, on account of the slimy nature of the insoluble matter. The solution so made contained in 100 cub. centims. 1·46 grm. of solid matter, calculated from the specific gravity (using the divisor 3·86); 1·0125 grm. of glucose, and caused an optical rotation of eight divisions in a 200 millims. tube. This gives a specific rotatory power, $\rho = \frac{8 \times 0{\cdot}242}{2 \times 0{\cdot}0146} = 66^{\circ}{\cdot}3$.

5 grms. of starch, previously dried at 100° C., were gelatinised with about 75 cub. centims. of water, the paste allowed to cool to 40° C., then mixed with 25 cub. centims. of the *kôji* solution, and left for

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* "Chemical News," xlii, p. 63.

† "Bull. Soc. Chim.," xxxi, 105.

‡ "Journal of the Chemical Society," 1879, Trans., pp. 600, &c.

twenty-five minutes, till it was quite clear. It was then rapidly heated to boiling, cooled, and diluted to 250 cub. centims.

100 cub. centims. of this solution, after filtration, contained 2·15 grms. of solid matter, and 0·63 grm. glucose, determined by weighing the cupric oxide after ignition. The optical rotation in a 200 millims. tube was 32·4 scale-divisions. As the *kôji* solutions contained in 250 cubic centims. was 25 cub. centims. (that is, one-tenth of the whole), we must deduct the weight of solid matter and glucose contained in 10 cub. centims. of the *kôji* extract from the weights above found in 100 cub. centims. of the liquid. The optical rotation must likewise be diminished by one-tenth the amount caused by the *kôji* solution alone. We thus get—

Solids in 100 cub. centims. formed from starch	2·15 − 0·146 = 2·004	grms.
Glucose (or its equivalent)....	0·63 − 0·101 = 0·529	„
Optical rotation	32·4 − 0·8 = 31·6	scale-divisions.

From which the observed specific rotatory power—

$$\rho = \frac{31\cdot6 \times 0\cdot242}{2 \times 0\cdot02004} = 190^\circ\cdot8.$$

The percentage composition calculated from the cupric oxide reducing power is

Maltose	43·28
Dextrin	56·72
	100·00

which requires $\rho = 187^\circ\cdot4$, taking maltose $= 150^\circ$, and dextrin 216°. The agreement between the observed and specific rotatory powers is sufficiently close to show that maltose and dextrin are produced. It may be remarked, in passing, that Brown and Heron's equation, No. 4, requires maltose $= 41\cdot3$ per cent., and $\rho = 188^\circ\cdot7$.

5 grms. of starch were gelatinised, and, after cooling to 40° C., mixed with 25 cub. centims. of the same *kôji* extract, and kept at that temperature for three-quarters of an hour. An additional 25 cub. centims. of *kôji* was then added, and the whole allowed to remain at 40° C. for a quarter of an hour longer, then boiled and diluted to 250 cub. centims. After filtration, the solution contained, deduction having been made as before for the *kôji*)—

Solid matter	2·035	grms. in 100 cub. centims.
Glucose (or equivalent)..	0·8882	„ „
Optical rotation.........	28·5	scale-divisions.

$$\therefore \rho \text{ (obs.)} = 169^\circ\cdot5.$$

The percentage composition, calculated as before, is

Maltose	71·54
Dextrin	28·46
	100·00

and calculated the specific rotatory power is $\rho=168^\circ\cdot8$.

With a solution prepared from different *kôji*, using 50 cub. centims. containing ·603 grm. of solid matter, the following results were obtained from 5 grms. of gelatinised starch kept for two hours at 10° to 15° C.

Maltose	70·00
Dextrin	30·00
	100·00

ρ (observed) .... $=174^\circ\cdot0$
ρ (calculated) .... $=169^\circ\cdot8$

The two last results correspond with Brown and Heron's equation No. 7.

$$10C^{12}H_{20}O_{10}+7H_2O=7C^{12}H_{22}O_{11}+3C^{12}H_{20}O_{10},$$

which requires 70·9 per cent. maltose, and $\rho=169^\circ\cdot2$.

An example may now be given of a solution containing both maltose and dextrose.

5 grms. of gelatinised starch were mixed with 56 cub. centims. of *kôji* extract, the same as in the last experiment, and allowed to stand at a temperature of from 10° to 15° C. for twenty-four hours. After diluting to 250 cub. centims., and making deductions for the *kôji* added, the composition of the solid matter was—

Maltose	52·8	per cent.
Dextrose	23·79	,,
Dextrin	23·41	,,
	100·00	,,

ρ (observed) .... $=143^\circ\cdot8$
ρ (calculated) .... $=143^\circ\cdot8$.

It may be observed, that the number of molecules of each body present is very nearly the same—a mixture of one molecule of maltose, one of dextrose, and one of dextrin, requiring

Maltose	50·00	per cent.
Dextrose	26·30	,,
Dextrin	23·70	,,
	100·00	,,

and $\rho=141^\circ\cdot7$.

From the experiments described in an earlier part of this paper, which showed that the extract of *kôji* caused the hydration of maltose, it might at once be accepted as a fact that longer digestion of the starch products with the *kôji* solution would result in the complete removal of the maltose, only dextrose and dextrin remaining, the latter being much more slowly hydrated than maltose. We cannot, indeed, expect any indications of definite chemical reactions occurring, because they would be disguised by the simultaneous hydration of the maltose and dextrin.

The following experiments illustrate the complete removal of the maltose. 20 grms. of dry starch gelatinised and 200 cub. centims. of a solution of *kôji* were diluted to one litre and heated at 40° C. for six hours, then allowed to stand for twenty hours at 15°C. The solution contained in 100 cub. centims. (deduction having been made for the *kôji* used) 1·96 grm. of solid matter and 1·68 grm. of glucose, and caused a rotation of 12·8 scale-divisions. This gives a percentage composition for the solid matter—

Dextrose	85·7	per cent.
Dextrin	14·3	„
	100·00	„

ρ (observed) .... $=79°$

ρ (calculated) .... $=81°·4$

4 grms. of gelatinised starch and 96 cub. centims. of *kôji* solution were heated to 35° C. for 3½ hours, then evaporated to about 200 cub. centims., and diluted to ¼ litre. The composition of the starch products in solution was—

Dextrose	86·00	per cent.
Dextrin	14·00	„
	100·00	„

ρ (observed) .... $=85°·7$

ρ (calculated) .... $=81°$

Without attaching much importance to the fact, it may be noticed that in the last two experiments the molecular ratio is about six of dextrose to one of dextrin; a mixture which would give—

Dextrose	87	per cent.
Dextrin	13	„
	100	„

and $\rho=79°·4$.

Having thus shown the nature of the reaction which occurs between starch-paste and the diastase of *kôji*, some experiments will next be described which will show the rapidity of the action at different temperatures and with differing proportions of *kôji* and starch. The first series of experiments was carried out at the ordinary temperature of the air, which at the time varied between 4° and 10° C. 450 cub. centims. of starch-paste containing 11·43 grms. of dry starch were mixed with 50 cub. centims. of *kôji* extract containing 1·786 grms. of solid matter. The mixture was allowed to stand at this temperature (4—10° C.) with an occasional shaking, and samples were withdrawn at the specified times. After making deductions for the *kôji* the amount of the starch products was found to be as follows:—

Table X.—Action of *Kôji* Extract on Starch at 4—10°C.

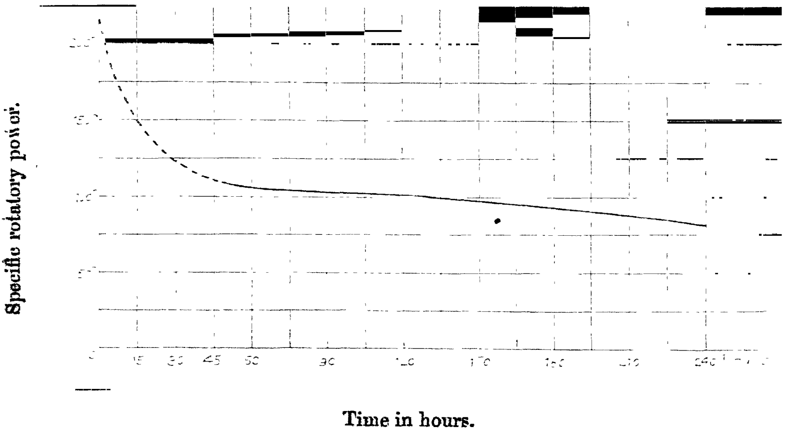
10 grms. starch to 8·75 grms. *kôji*.

Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used to form the extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	°
48 hours	11·43	10	9·714	109·6
120 „	„	„	9·904	100·2
192 „	„	„	10·369	90·4
240 „	„	„	10·450	80·4

The course of this reaction is represented graphically in Curve B. The action, comparatively rapid at first, goes on slowly but con-

CURVE B.

Action of *kôji* extract on starch at 4°—10°.



tinuously to the end of the experiment, which lasted ten days. The liquid remained quite clear to the end, the temperature being too low for the development of organic life. In a second series of experiments conducted at the same temperature, with different proportions of starch and *kôji*, corresponding results were obtained.

Table XI.—Action of *Kôji* Extract on Starch at 4—10° C.

10 grms. starch to 40 grms. *kôji*.

Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used, as extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	
68 hours.....	5·08	20	4·638	100°·4
164 „	„	„	4·816	75·3

The influence of a larger proportion of *kôji* is seen in these experiments, for whereas in the first series 8·75 grms. of *kôji* were used to 10 grams of starch, in the second series the weight of *kôji* was 40 grms. to the same weight of starch. It will be seen that in 164 hours the larger proportion of *kôji* used has effected a reduction in the specific rotatory power greater than in 240 hours, with the smaller proportion, but the rapidity of change does not seem to be proportional to the quantity of *kôji* used.

The next two series were carried out in exactly the same way, but the temperature of the air varied between 10° and 15° C.

Table XII.—Action of *Kôji* Extract on Starch at 10—15° C.

10 grms. starch to 10 grms. *kôji*.

Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used, as extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	
½ hour........	10	10	10·61	172°·8
2 hours......	„	„	10·45	158
21½ „	„	„	10·56	131
26 „	„	„	10·65	120·4
46 „	„	„	10·51	110·5

The next series was conducted exactly as the one just described, but samples were not taken until 48 hours after the first hour.

Table XIII.—Action of *Kôji* Extract on Starch at 10—15° C.

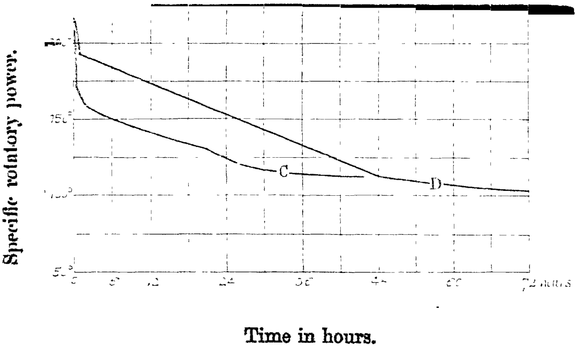
10 grms. starch to 10 grms. *kôji*.

Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used, as extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	
1 hour	10	10 .	9·705	193°
48 hours......	,,	,,	9·925	112
72 ,, ...	,,	,,	9·925	102·7

The results of these experiments are shown graphically in Curves C and D. In the last series, Table XIII, at the end of 72 hours the reduction in the specific rotatory power is not much greater than in the previous experiments after 48 hours, although to all appearance the conditions were identical. But it is probable that different portions even of the same lot of *kôji* differ somewhat in activity. Another possible cause of difference is suggested by Curve C. The inclination of that curve between 21½ hours and 26 hours is much greater than the average inclination after the first rapid fall is over. This was undoubtedly due to the temperature between these two observations having been higher than the average taken over the whole time. During the greater part of the time the temperature was not higher than 10° C., except in the middle of the day, when it stood about 15° C. The two observations at 21½ hours and 26 hours were made

Curves C and D.

Action of *kôji* extract on starch at 10°—15° C.



in the middle of the day, and thus the more rapid action is explained. In the same way, the average temperature during the series first mentioned may have been more nearly 15° C. than during the last series, although I do not know this to have been so. It is, at any rate, a possible cause of difference.

No certain conclusions can, I think, be drawn as to the reaction being expressed by any definite chemical equation; there is at first in all cases a very rapid fall; and, in Curve C the specific rotatory power, after twenty minutes, nearly agrees with that required by Brown and Heron's equation No. 7, before alluded to; but, after this, the absorption of water by the maltose would make the curve very regular, and it would show no breaks in its continuity.

The two following series of experiments were conducted at a temperature of 40° C., the flask containing the mixture being immersed in a water-bath kept at that temperature, and samples were taken at certain intervals of time. The solution used for determining the total solid matter from its specific gravity was rapidly cooled by means of ice, and the other portion in which the specific rotatory power was to be determined was poured into a dry flask containing a little salicylic acid, as recommended by Brown and Heron, and also rapidly cooled. Deduction was made for the amount of *kôji* solution added, as in the previous experiments. The two series of experiments differ only in the relative proportions of *kôji* and starch used—that in the first series being 5 grms. of *kôji* to 10 grms. of starch; and, in the second, 10 grms. of *kôji* to the same weight of starch.

Table XIV.—Action of *Kôji* Extract upon Starch at 40° C.

10 grms. of starch to 5 grms. of *kôji*.

Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used, as extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	
25 minutes..........	10	5	10·08	167
4 hours.............	,,	,,	10·08	127
+ 92 hours at 15° C..	,,	,,	10·25	106

Table XV.—Action of *Kôji* Extract upon Starch at 40° C.

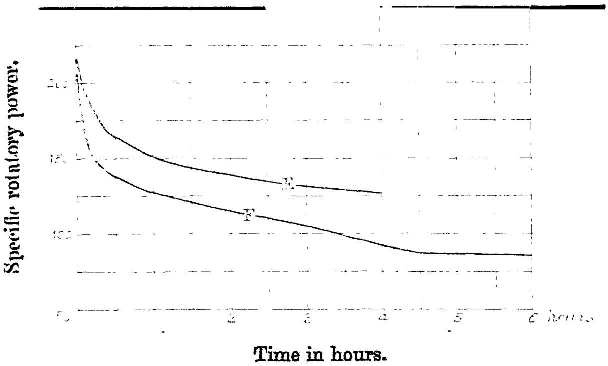
10 grms. of starch to 10 grms. of *kôji*.

Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used, as extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	°
⅓ hour..............	10	10	9·64	143·1
1 "	"	"	9·64	127
2 hours	"	"	9·64	115
3 "	"	"	9·65	105
4½ "	"	"	9·67	88
6 "	"	"	9·69	86
+ 20 hours at 15° C..	"	"	9·79	80

The preceding results are represented graphically by Curves E and F. The influence of a greater proportion of the *kôji* solution will again be noticed in the much greater fall in specific rotatory power in a given time; and further, that in neither of these curves is any very sudden bending to be observed. The inclination of the curve during the first half an hour is certainly greater than it is afterwards; but, although in neither case has the activity of the diastase been destroyed, the bending does not occur at the same specific rotatory power. This is doubtless owing to the action of the diastase upon the maltose first formed, which at this temperature is rapidly hydrated to dextrose. The hydration is more rapid in the case of the experiments represented by Curve F, although in other respects the two curves show a remarkable similarity.

Curves E and F.

Action of *kôji* extract on starch at 40° C.



The next table gives the results of an experiment carried out at

45° C., 10 grms. of dry starch, and the extract of 10 grms. of *kôji* being contained in 500 cub. centims.

Table XVI.—Action of *Kôji* Extract on Starch at 45° C.

10 grms. of starch to 10 grms. of *kôji*.

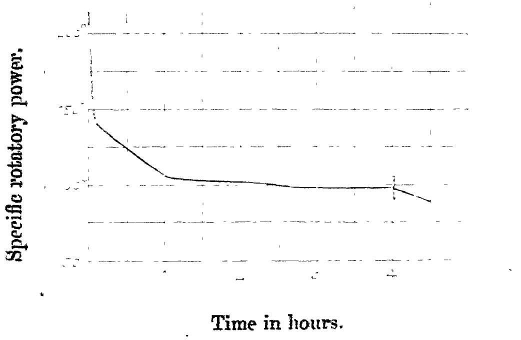
Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used, as extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	
5 minutes…………	10	10	9·48	142°·6
25 " …………	"	"	9·93	126·3
1 hour…………	"	"	9·93	106
1½ " …………	"	"	..	103·6
2 hours …………	"	"	9·98	103
3 " …………	"	"	9·98	98·3
4 " …………	"	"	..	98·3
Fresh *kôji* added.				
4½ hours…………	"	"	10·18	88

These results are represented graphically by Curve G. The fall is very rapid for the first five minutes, until a specific rotatory power of 142°·6 is attained, then the curve follows an almost straight line till one hour has elapsed, a specific rotatory power, $\rho=106°$ being attained. After this it follows an almost horizontal path until a fresh addition of *kôji* extract was made, when it again falls, the inclination being almost the same as between five minutes and one hour. This shows that the cessation at 106° was due to the exhaustion of the *kôji* extract, and not to the existence of any definite chemical equation corresponding to $\rho=106°$.

Curve G.

Action of *kôji* extract on starch at 45° C.

(The thick vertical line indicates fresh addition of *kôji* solution.)



The last series I shall give contains experiments made at a temperature of 60° C., the proportions of starch and *kôji* being the same as in the last series.

Table XVII.—Action of *Kôji* Extract upon Starch at 60° C.

10 grms. of starch to 10 grms. of *kôji*.

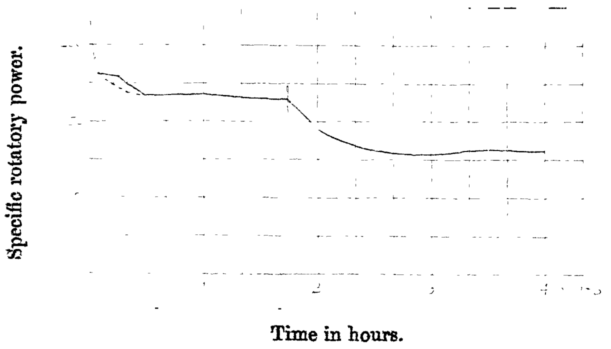
Time.	Dry starch in 500 cub. centims.	Weight of *kôji* used, as extract.	Total starch products.	Specific rotatory power.
	Grms.	Grms.	Grms.	°
5 minutes...........	10	10	9·70	182·1
15 ,, ...	,,	,,	,,	180·2
30 ,, ...	,,	,,	,,	168
1 hour..............	,,	,,	,,	168
1½ ,, ...	,,	,,	,,	164·6
1¾ ,, Fresh *kôji* added	10·02	20	,,	
2 hours............	,,	,,	10·19	145·8
2½ ,, ...	,,	,,	,,	131·8
3 ,, ...	,,	,,	,,	128·2
3½ ,, ...	,,	,,	,,	131·8
4 ,, ...	,,	,,	,,	131·8

Curve H represents these results in a graphic manner. It is probable that the result given for fifteen minutes is incorrect; there appears to be no good reason for the curve deviating from the expected course as it does. After the lapse of thirty minutes the specific

Curve H.

Action of *kôji* extract upon starch at 60° C.

(The thick vertical line indicates the addition of fresh *kôji* solution.)



rotatory power remains practically stationary at 164—168° for 1¾ hour, then, when a fresh addition of the *kôji* extract was made, the curve rapidly falls to 131°·8, at which point it again remains fixed, if we assume, as is probable, that the result at three hours is erroneous. The first stoppage, and doubtless also the second, was caused by the exhaustion of the activity of the *kôji*, and not to the decomposition of the starch at that point according to any definite chemical equation, because the action goes on immediately after the addition of a fresh quantity of diastase, and ceases after about the same time.

At 60° C., therefore, the activity of the diastase is very quickly destroyed, and at 70° C. the action is so small that no satisfactory determinations could be made. The extract of 10 grms. of *kôji*, previously heated to 70° C., was added to starch-paste containing 2 grms. of dry starch. After fifteen minutes the solution diluted to 250 cub. centims. only contained 0·40 grm. of solid matter, and caused a scarcely perceptible rotation of the ray of polarised light. We may, therefore, conclude that at some point between 60° and 70° C., the diastase of *kôji* is destroyed.

The experiments described do not allow us to conclude that the starch molecule breaks up in a definite manner under the influence of *kôji* diastase, as appears to be the case with the starch molecule under the influence of malt diastase (O'Sullivan, Brown and Heron, &c.), but they do not contradict the supposition, and the results are just such as might be expected, knowing the comparatively energetic action which the *kôji* solution has upon dextrin, and especially upon maltose.

The diastase of *kôji* resembles that of malt in one respect, that its activity is lessened by the presence of certain bodies, such as common salt. In preparation for an investigation into the chemistry of the "Soy" manufacture, Mr. Watanabe made some experiments upon the influence of varying amounts of common salt upon the activity of the *kôji* solution. In each experiment 5 grms. of dry starch were gelatinised, and when cold the given amount of common salt was added. The whole occupied about 150 cub. centims. The required amount of *kôji* extract was then added and the mixture left for one hour. At the end of that time the solution was diluted to 250 cub. centims. and filtered. The results give the cupric oxide reducing power of the solid matter in solution, and the specific rotatory power, both corrected for the salt and *kôji* extract previously added.

Table XVIII.—Action of *Kôji* Extract upon Gelatinised Starch in presence of Common Salt.

Common salt used per cent. of starch.	Cupric oxide reducing power.	Specific rotatory power.
0	30·8	173°·8
10	28·6	179·3
30	25·1	182·6
50	23·8	187·6
75	20·9	190·3
100	20·1	189·1
150	19·1	190·2
200	18·0	192·2
300	16·9	194·1
500	14·4	197·5

The diminution in the cupric oxide reducing power, and the increase in the specific rotatory power, corresponding with an increased proportion of salt present, are so marked and so uniform that no further remarks are needed.

No other observations have, to my knowledge, been made upon the effect of moulds in transforming starch into sugar, although M. Gayon (" Compt. Rendus," tom. 86, p. 52) has shown that cane-sugar is inverted by certain fungi, such as *Penicillium glaucum* and *Aspergillus niger*.

M. Pasteur, in his interesting work on " Beer,"* has drawn attention to the possible employment of moulds in industry on account of the power they possess of destroying organic matter. He says :— " Un jour viendra cependant, j'en suis persuadé, où les moisissures interviendront dans certaines opérations de l'industrie par leur propriétés de destruction de la matière organique." And in a note, " Je montrerai un jour que les combustions dues aux moisissures provoquent dans certaines putréfactions des dégagements considérables d'ammoniaque, et qu'en réglant leur action on pourrait les faire servir à retirer, sous cette forme, l'azote d'une foule de débris organiques, comme aussi, en empêchant la production de ces petites plantes, on pourrait accroître beaucoup la proportion des nitrates dans les nitrières artificielles. En entretenant humides des morceaux de pain dans un courant d'air et cultivant à sa surface diverses sortes de moisissures, j'ai pu faire dégager des torrents d'ammoniaque à la suite de la combustion par ces mêmes moisissures des matières hydrocarbonées. La putréfaction des asperges et celle de beaucoup d'autres substances animales ou végétales m'ont donné des résultats ana-

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* " Etudes sur la Bière," p. 253. Also note on p. 254.

logues." The industrial employment of fungi under consideration is an illustration of this, for, although the organic matter of the rice is not completely destroyed it is removed to some extent in the form of carbonic acid, and the albuminoids, though not converted into ammonia, are undoubtedly degraded, and brought into such a form that they can be dissolved out in great measure by water. It is doubtless to the presence of albuminoids in this form that *kôji* owes its remarkable action upon gelatinised starch.

IV.—*Change which the Rice Grain Undergoes by the Growth of the Fungus.*

What is the nature of the change which occurs in the conversion of rice into *kôji?* As the grains have been exposed for a period of four or five hours to the vapour of boiling water, and further, as the embryo has been completely removed, it cannot be of the nature of germination. The use of the fungus-spores and the growth of the mycelium indicate that the alteration in properties is connected with the growth of the mould, and it is probably of a similar character to that described by Brown and Heron, which takes place in a solution of barley under the influence of the yeast cell. They say: "An aqueous extract of barley, which is submitted for a few hours at a temperature of 30° C. to the action of ordinary yeast has its power of transforming starch much increased by this treatment . . . . . It is evident, that the growing yeast cell is capable of inducing certain modifications in the albuminoids which, during the ordinary process of germination, are brought about by the action of the living vegetable cell itself."*

In this case we may suppose that the yeast cell in absorbing nitrogenous food, does so by attacking the insoluble albuminoids contained in the barley, and having taken what it requires, leaves the rest behind in a degraded state, in such a form that water easily dissolves it. In the formation of *kôji*, we may assume that something of the same sort occurs, the growing fungus attacks the insoluble albuminoids of the rice, and in proportion as they are brought into a soluble state, so does the diastatic property of the rice increase. The principal aliment of the fungus is, doubtless, the starch granule, and this will be found to be the case on examining the grain microscopically. The outer layers of the *kôji* grain are loose; the mycelium penetrates between the cells everywhere, but in the centre the cells are horny, and no starch granules are to be distinguished.

The chemical change which takes place may be seen by comparing the analysis of rice with that of *kôji*. The rice, before the removal of the thin skin, has been examined by a number of chemists, and as all the analyses very nearly agree, the grain may be considered not to vary very much in composition. The following is one of many analyses made in this laboratory, of the rice newly grown.

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* "Journal of the Chemical Society," Trans., p. 653, 1879.

Analysis of Husked Rice Dried at 100° C.

Starch, including sugar and dextrin ..	83·20	per cent.
Fat	1·21	„
Albuminoids	8·10	„
Cellulose (by difference)	6·58	„
Ash	0·91	„
	100·00	„

Water in original grain, 11·96 per cent.

The outer cells, which are removed during the cleaning, are much richer in nitrogen than the rest of the grain. A sample analysed in the University Laboratory, contained 16·7 per cent. of albuminoids calculated on the dried substance, and other observers have found from 15 to 16·4 per cent. It therefore contains about twice as much as the average of the whole husked grain, which must be rendered proportionately poorer by its removal. In one sample of the cleaned rice used in making *kôji*, the amount of albuminoids calculated on the dried grain was found to be 6·47 per cent.

The following is a complete analysis of *kôji;* unfortunately I have not the analysis of the rice from which it was made to compare with it.

Composition of Kôji *Dried at* 100° C.

Soluble in water=37·76 per cent.:—			
Dextrose.....................	25·02	per cent.	
Dextrin (by difference)	3·88	„	
Soluble ash	0·52	„	
Soluble albuminoids..........	8·34		Total albuminoids 9·84 per cent.
Insoluble in water=62·24 per cent.:—			
Insoluble albuminoids	1·50		
Insoluble ash................	0·09	per cent.	
Fat	0·45	„	
Cellulose	4·20	„	
Starch (by difference)	56·00	„	
	100·00	„	

Water in fresh *kôji*, 25·82 per cent.

The chief point which a comparison of these analyses brings out, is the large proportion of the total albuminoids which is dissolved by water from *kôji*. Referring always to the material dried at 100° C., the total albuminoids in *kôji* amount to 9·84 per cent., whilst 8·34 per cent. was found in solution. In the case of rice, the amount of albuminoids dissolved by water is very small; in one specimen of cleaned rice examined, it was 1·38 per cent., the total albuminoids amounting to 6·47 per cent. It is evident, therefore, that one change effected by the growth of the fungus is to increase the proportion of soluble nitrogen.

If the total amount of albuminoids in *kôji* be compared with the amount in rice, it will be seen that it is greater in the former than in the latter, evidence of the removal of the carbohydrates, alluded to in Section I.

The broken grains which result from the cleaning of the common rice are also converted into *kôji*. As the bran is mixed with the broken grains, the whole contains a larger proportion of albuminoids than the original rice. The following analyses were made of the broken grains, and of the *kôji* formed from them. In both cases the results are given upon the material dried at 100° C.

Table XIX.—Composition of *Kôji* prepared from Broken Grains.

	Soluble solid matter.	Dextrose.	Soluble albuminoids.	Insoluble albuminoids.
	Per cent.	Per cent.	Per cent.	Per cent.
Broken rice......	5·04	..	1·47	7·33
Kôji...........	25·90	12·80	7·08	

The amount of solid matter dissolved from the *kôji* is just about five times as much as that dissolved from the rice, and the soluble albuminoids have increased in nearly the same proportion.

Beyond the recognition of this alteration in the solubility of the albuminoids, and the related increase in the amount of solid matter dissolved, I have been able to come to no definite conclusion. I hope, however, to have the opportunity of examining more in detail the nature of the individual albuminoids present in *kôji*, but thus far, that is a part of the problem I have not touched. It is, indeed, generally believed, that the converting effect of diastase is owing to the existence of certain albuminoids in solution, and the results obtained in this research go to show that the active properties of *kôji* are accompanied by the presence of soluble albuminoids, but to go beyond this, and to show how, by the growth of the fungus, this change is effected, is a problem which, however interesting, lies beyond my power.

Having drawn attention to the fact that a particular fungus has this power of rendering the rice grain diastatic, an effect which has hitherto been attributed only to the germination of the embryo, the question whether the effect is a general one or not, must be left to professed vegetable physiologists.

In conclusion, the pleasant task remains to me of expressing my obligations, and offering my thanks to Hiroyuki Kato, Esq., President of the University of Tôkiô, who has rendered my task a comparatively easy one, by the assistance he has given me in various ways. To Mr. Jihei Kameyama also, the proprietor of the *kôji* manufactory in Yûshima, Tôkiô, I am deeply indebted for the willingness with which I have been allowed to make experiments, and to collect information in his works. I wish also to thank my assistant, Mr. Nakazawa, for the interest he has taken in the research, and for much assistance which I have received from him.

June 3, 1881.

The Annual Meeting for the Election of Fellows was held this day.

THE PRESIDENT in the Chair.

The Statutes relating to the election of Fellows having been read, Sir Joseph Fayrer and Mr. J. W. Hulke were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society.

Ayrton, Prof. William Edward.
Bates, Henry Walter.
Bristowe, John Syer, M.D., F.R.C.P.
Christie, William Henry Mahoney, M.A., Sec. R.A.S.
Dickie, Prof. George, A.M., M.D., F.L.S.
Kempe, Alfred Bray, B.A.
Macalister, Prof. Alexander, M.D., Sec. R.I.A.
McLeod, Prof. Herbert, F.I.C., F.C.S.
Phillips, John Arthur.
Preece, William Henry, C.E.
Samuelson, Bernhard, M.I.C.E.
Stoney, Bindon Blood, M.A., M.I.C.E.
Traquair, Ramsay H., M.D.
Watson, Rev. Henry William, M.A.
Wright, Charles R. Alder, D.Sc.

Thanks were given to the Scrutators.

June 16, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor William Edward Ayrton, Mr. Henry Walter Bates, Dr. John Syer Bristowe, Mr. William Henry Mahoney Christie, Professor Herbert McLeod, Mr. John Arthur Phillips, Mr. William Henry Preece, Rev. Henry William Watson, and Dr. Charles R. Alder Wright were admitted into the Society.

The following Papers were read:—

I. "On the Differences in the Physiological Effects produced by the Poisons of certain species of Indian Venomous Snakes." By A. J. WALL, M.D. (Lond.), Surgeon H.M. Indian Army. Communicated by Sir JOSEPH FAYRER, M.D., K.C.S.I., F.R.S. Received May 2, 1881.

Hitherto no clear distinction has been recognised to exist between the actions of the poisons of the various species of venomous reptiles. It will be the object of this paper to examine closely the symptoms produced by these poisons in order to detect any differences that may be present, and to see if the poisonous agents can be classified according to their physiological effects. As poisonous reptiles admit of a simple anatomical classification into colubrine and viperine, one member of each group will be taken. It will be convenient to begin with the cobra, which is one of the most virulent and best known of colubrine venomous snakes.

I.—*The Physiological Effects of the Poison of the Cobra* (Naja Tripudians).

In order to exhibit the effect of this venom, it will be necessary to detail the results that follow when animals of different classes have been poisoned by this snake.

Experiment I.

At 10.46 A.M., a pariah dog was bitten in the thigh by a cobra.
11.14 A.M. Very lame in the bitten limb.

11.27 A.M. Affected by the poison. Staggers when he attempts to walk. Chewing movement of the jaw and lips.

11.28 A.M. Salivation.

11.30 A.M. Pupils somewhat small.

11.31 A.M. Attempting to vomit.

11.35 A.M. Respiration becoming slower.

11.37 A.M. Respirations 16 per minute.

11.38 A.M. Copious salivation.

11.39 A.M. Respirations fallen to 12 per minute.

11.42 A.M. Tongue hanging out of the mouth.

11.43 to 11.46. Convulsions.

11.47½ A.M. Respiration completely ceased. Pupils dilating.

11.49 A.M. Heart stopped. Dead.

Experiment II.

A fine large cock had 2 mgrms. of dried cobra poison in solution injected into its leg.

3.13 P.M. Injection.

3.20 P.M. Respirations 30 per minute.

3.38 P.M. Head drooping as if the neck were too weak to support it; but from time to time the head is raised with a jerk.

3.41 P.M. Respirations 25; can barely stand.

3.48 P.M. Respiratory movement very slight.

3.50 P.M. Cannot stand.

3.55 P.M. Respirations 19; pupils somewhat contracted.

4.3 P.M. Respirations 16; movement exaggerated.

4.5 P.M. Comb has become of a dusky purple colour; slight convulsive movements of the body.

4.9 P.M. Respirations 9.

4.17 P.M. Convulsions.

4.20 to 4.22 P.M. Convulsions continuing, but gradually becoming less violent.

4·25 P.M. Pupils widely dilated. Dead.

Experiment III.

A medium-sized frog (*Rana tigrina*) had 5 cgrms. of dried cobra poison dissolved in water, injected into its dorsal sac:—

12.42 P.M. Injection.

1.23 P.M. Struggling violently to escape.

1.40 P.M. Becoming paralysed.

1.53 P.M. Dead.

In these three experiments the effects of cobra poison on mammals, birds, and amphibia, are well shown. But as the symptoms in man, owing to the differences in his nervous system, are peculiar, it is requisite to give an outline of the results of cobra poisoning in his case. The one selected is condensed from Dr. Hilson's account ("Indian Medical Gazette," October, 1873).

A punkah coolie was bitten by a cobra on the right shoulder about half-past 12 o'clock at night. He immediately felt an acute burning pain on the spot, which increased in severity. A quarter of an hour afterwards he said he was beginning to feel intoxicated, but seemed quite rational, and answered questions intelligently. The pupils were natural, the pulse normal, and the respiration was easy. He next began to lose power in his legs, and staggered. Half an hour after he was bitten his lower jaw began to fall, and frothy and viscid saliva to run from his mouth, and he spoke indistinctly, like a man under the influence of liquor, and the paralysis of the legs increased. Forty minutes after the bite he began to moan and shake his head from side to side, and the pulse and respiration were somewhat accelerated.

He was unable to answer questions, but appeared to be quite conscious, and his arms were not paralysed. The breathing then became slower and slower, and finally ceased about one hour and ten minutes after the bite, the heart beating for about one minute longer.

From these cases, and from the evidence given by other experiments, we can draw up a summary of the chief facts to be noticed during cobra poisoning. The first manifestation of cobra poison having been injected beneath the skin is a sensation of pain in the bitten part. The evidence of pain occurring in animals is very clear; the animal turns and licks the spot, and if it is the leg that is wounded it either limps on that leg or, what is more usual, draws it up so as to ease it.

This action has been termed paralysis of the bitten leg, due to the local contact of the poison with the muscles. Now, though it can be proved that the local effect of cobra poison on muscle is to weaken it, yet after the bite of a cobra a very small extent indeed of muscle comes in contact with the poison—very often none at all; and if the limb were really paralysed it would hang uselessly down, dragging upon the ground, instead of being drawn up.

This pain is accompanied by, or rather is dependent on, a very characteristic local condition, that is worthy of careful attention. If the body of a man or animal killed by snake-bite be examined, there may be scarcely a sign to mark the spot where the snake bit—a scratch or puncture may apparently be the extent of the injury. If an incision be made through the skin and carried through the punctures, very little change will be found in the true skin. It may be somewhat more injected with blood than normal, and the punctures will be found to be intensely so just at their edges, and a small quantity of blood may be effused there. But the areolar tissue lying beneath the true skin is the site of the chief changes. It will frequently be found to be of a purple colour, and to be infiltrated with a large quantity of coagulable purple blood-like fluid. In addition the whole of the neighbouring vessels are intensely injected. This injection gradually lessens as the site of the poisoned part is receded from, so that a bright scarlet ring surrounds the purple area, and this, in its turn, fades into the normal colour of the neighbouring tissue. At the margin also the purple blood-like fluid is replaced by a pinkish serum, which may often be traced up in the tissues surrounding the vessels that convey the poison to the system. In one case in which the victim was bitten on the hand, I traced this effusion around the veins as high as the elbow. The local appearances differ considerably in different cases, varying from those excessive ones just described to a mere hyperæmia. It has been asserted that these changes are merely the result of hæmorrhage from the divided vessels. But this will not account for the pain or for the intense injection of the surrounding parts, or for the fact that should the bite not prove fatal the site of it nearly always suppurates. The real explanation is that cobra poison is an intense irritant and produces acute inflammation. Thus, if a small quantity be placed in the eye, the most severe inflammation of the conjunctiva follows. The existence of this local inflammation is of great value. It takes place with startling rapidity, one minute or less producing marked hyperæmia. This local hyperæmia is, therefore, the first indication that we can obtain that snake poison has really entered the system. As life itself depends upon the rapidity with which snake poisoning can be recognised, it will be seen that this is a matter of the greatest practical importance.

An interval now occurs before any fresh symptom is noticed, but the length of it varies greatly in different cases. In dogs bitten by cobras the average of four experiments gave 18·2 minutes as the length of this interval.

It is not possible to determine with exactitude the average length of this period in man, but in the case given it was fifteen minutes, in another instance reported by the same observer it was an hour and fifteen minutes; and an eye-witness, who evidently described a case with great accuracy as to details, stated four hours passed before any change was noticed. The evidence available on the subject makes it probable that an interval of an hour is the average of this period in man.

This pause, it can be proved, depends on two separate factors. The one factor is the time required for the absorption of the poison; for it is lessened by the poison being injected simultaneously into several different sites. The other is clearly dependent on some secondary change produced by the poison for which time is necessary; for if the part into which the poison has been injected be excised before the occurrence of a single constitutional symptom, yet, nevertheless, the animal may die apparently as rapidly as if no interference had been attempted; showing that the mere presence of the poison in the blood, even in sufficient quantity to kill, is not capable of producing directly a physiological effect. On the other hand, to prevent grave misconception, it should be stated that it is quite possible to save life by excision of the bitten part, if it be done sufficiently quickly to prevent any considerable absorption.

The symptoms once developed follow one another with great rapidity. In man a feeling of intoxication appears to be the first constitutional effect of the poison. It is very generally complained of, but not universally, as it would require some intelligence on the victim's part to mention it. It is not possible to get evidence of a purely subjective condition in animals.

In man the next symptom is loss of power in the legs. There is first staggering, then inability to support the body, and finally there is complete incapacity to move the lower limbs. At the same time there is scarcely any loss of power in the arms, which may remain completely under the influence of the will. The exact nature of the action of cobra poison on the nervous system is, however, a very difficult subject. Sir Joseph Fayrer and Dr. Brunton, in their valuable series of papers on the subject, maintain that though the greater part of the nervous system is affected, yet the terminations of the motor nerves suffer especially, and in a very marked manner. They base their reasoning on the results produced by experiments in which the excitability of two nerves of the same animal is tested, one of which has been subjected to the action of the poison, and the other has been kept from the contact with the poisoned blood by the limb to which it is distributed being ligatured, the nerve, however, being left intact. These experiments I have repeated with, however, some differences in the arrangements, with the result that though the poisoned limb lost its excitability to a very much greater extent than the non-poisoned limb, yet the spinal cord, as long as it was capable of stimulation at all, could convey stimulation to the poisoned and unpoisoned nerve, but that the excitability of the cord was exceedingly quickly lost. These results would imply that the terminations of the motor nerves only suffered, *pari passû*, with the cord itself, and that there is no special elective affinity for the endings of the nerves. Nor are the results of the experiments of Sir Joseph Fayrer and Dr. Brunton incompatible with this view. For when one thigh of the subject of the experiment was ligatured, and the other was poisoned, when the cord was excited by a current, the stimulus had to be transmitted to the non-poisoned leg through the trunk of the nerve which was unaffected; whereas on the other leg it had to overcome the resistance induced by the paralysing poison. There is no need to suppose a special effect of the poison on the ends of the motor nerves; the different lengths of the trunks affected would account for a considerable difference. To this we have also to add the paralysing effect on muscle, which, though not so great as on the nerve, is yet not unimportant, and would tell on the same side. Moreover, it does not follow that because a nerve to which poison has had access conveys electrical *stimuli* in a very imperfect manner, or not at all, therefore the effect of that poison has been to paralyse the nerve. It is unfortunate that the only test we have of the vitality of a nerve is its power of causing contraction in a muscle when irritated by electricity or mechanically.

It would be going too far to say, therefore, that because a nerve did not transmit such rude *stimuli* it was dead; and when an animal loses the power of withdrawing a member that is being painfully stimulated, the break in the power of conducting impressions or *stimuli* may be in any part of the nervous chain involved, or may be distributed equally throughout. A poison also that produces death by totally different means than paralysis may yet cause in the nerves a complete deadness to *stimuli*.

Experiment IV.

The right thigh of a frog (*Rana tigrina*) was ligatured, so as to completely prevent circulation through the limb, the nerve being included in the ligature. Two cgrms. of strychnia in solution were then injected into its dorsal sac.

12.53 P.M. Injection.

12.56 P.M. Tetanus.

1.10 P.M. Reflex action ceased.

1.23 P.M. Muscles of right leg infinitely sensitive.

1.25 P.M. Muscles of left (poisoned) leg contract with 0·75 volt.

1.39 P.M. Right (unpoisoned) sciatic nerve infinitely sensitive, causes muscular contraction with less than ·0001 volt.

Now strychnia certainly does not kill by paralysis, and yet the difference between the poisoned and the non-poisoned sides in regard to their nerves was more marked than was recorded in similar experiments made with cobra poison. In another frog this difference was very pronounced before the strychnia had ceased to produce tetanus, o that it occurs long before exhaustion has taken place. The complete interference with the vital functions produced by tetanus is the real cause of death, and the deadening of the nerves is simply the result of the excessive nervous discharges that have taken place through them. Thus, though the trunk or extremity of a nerve may be found paralysed, it does not follow that it is the direct action of a poison that may be present, or that it was the paralysis that caused death.

In cobra poisoning also it is possible to get very distinct evidence of sensation long after the nervous centres of organic life have been so completely destroyed by poison, as to render the animal dependent on artificial respiration for life.

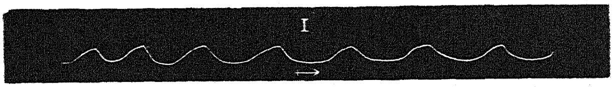
In this direction another point must be taken into account. It has already been stated that one of the most characteristic features of cobra poisoning in the human subject is paralysis of the legs. The patient is unable to walk or to stand, though his arms have not as yet experienced any loss of power. Now, it would be difficult to suppose that this was due to the terminations of the motor nerves of the legs becoming paralysed, while those of the arms remain unaffected. It is much more probable that the spinal cord is becoming paralysed, one of the first effects of which would be that it would lose the power of maintaining the tone and necessary contraction in the many complex groups of muscles on which the upright posture is dependent. But in cobra poisoning in dogs paralysis of the hind limbs without the fore is rarely seen. In the vast majority of cases power is lost simultaneously in all four members. In those few cases in which it has been noticed that the hind legs have suffered first, the animal has been bitten on the hind leg, which would always cause a certain amount of lameness and difficulty in walking with the hind quarters, due to the local effect of the poison. But even with this source of fallacy it holds good that in men suffering from cobra poisoning paraplegia is a most constant symptom, whereas it is very exceptional in dogs. The reason of this great dissimilarity is to be found in the different functions of the inferior portion of the spinal cord in the two cases. In man the lower portion of the cord is, to a great extent, a distinct nervous centre. The ganglia there not only are the centres on which the lower limbs rely for their nerve power to maintain the upright posture, but the sensation of contact with the ground of one foot in walking is translated in these lower centres into a motor stimulus to excite movement in the other leg, and paralysis affecting these centres would at once destroy the process. But in dogs the mechanism is very different. They move the foreleg of one side with the hindleg of the other. It is necessary, then, that the centres governing the movements in these limbs should be coupled—so to speak—together. The stimulus that moves the foreleg of one side has to excite simultaneous movement in the hind leg of the other. Therefore the posterior extremity of the cord has in the dog merely to transmit the motor impulse from the forepart, whereas in man it has to translate sensations into stimuli to excite movement, and this in man is the first faculty destroyed by cobra poison. It is, therefore, probable that the earliest injury inflicted on the nervous system by cobra poison is a paralysis of the centres in the lower part of the spinal cord.

The next symptoms of cobra poisoning are very characteristic. The patient loses power of speech, of swallowing, of moving the lips, the tongue becomes motionless and hangs out of the mouth, and the saliva which is secreted in large quantities runs down the face, the patient being equally unable to swallow it or to eject it. It is singular that the striking resemblance of these symptoms to the disease known as glosso-laryngeal paralysis has not been previously noticed. Now, the preponderance of opinion attributes this latter disease to lesion of certain tracts in the medulla oblongata. It cannot, therefore, be thought anything but reasonable to connect both diseases with paralysis of those centres in the medulla oblongata which are so closely associated together, and which are in connexion with the roots of the vagus, the spinal accessory, and the hypoglossal nerves, and the lower nucleus of the facial. But the resemblance does not end here. In both diseases the respiration becomes feebler and feebler, and the victim at last dies suffocated. In other words the lesion in the one case, and the paralysing poison in the other, have invaded the respiratory nucleus so near to the centres they have already destroyed, and have thus rendered the respiratory act difficult, and at last impossible. Lastly, after all the lower centres have been completely paralysed, the one by which connexion is made with the second, fifth, and seventh nerves still acts, and the eye is closed when touched, and even when approached, after the animal is dependent on artificial respiration for life. For these reasons it seems natural to conclude that the principal action of cobra poison on the nervous system consists of an extinction of function, extending from below upwards, of the various nerve-centres constituting the cerebro-spinal system; but in addition to this, there is a special and rapid action on the respiratory and allied nuclei, and it is to this special action that death is to be attributed in most cases of cobra poisoning. In very rapid cases of poisoning, when a very large quantity of poison has entered the circulation at once, instead of the gradual extinction of function of the cerebro-spinal centres, the poison appears to act almost immediately by stopping the action of the respiratory centre. There is, of course, no time then to watch the gradual extension of the influence of the poison on the nervous system. In these cases, the slight stimulation of the centres which almost always precedes the paralysis, instead of being represented by slight irregular contractions of the muscles, is exaggerated into violent clonic convulsions, but they are almost instantly followed by complete paralysis.

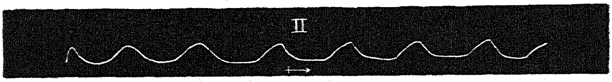
The action of the cobra poison on the respiration is of the deepest importance in the light it throws on the special action of the poison, and its relation to other poisons.

The first change that is noticed in the breathing of an animal after the introduction of cobra poison, is a decided quickening and deepening of the respiratory movements. Sir Joseph Fayrer and Dr. Brunton have shown that this effect is no longer to be perceived after section of both vagi. The inhibitory effect of section of the vagi may be too power-

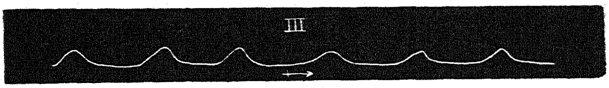
No. 1.—Tracings of Respiratory Movements of a Fowl under gradual Cobra Poisoning.



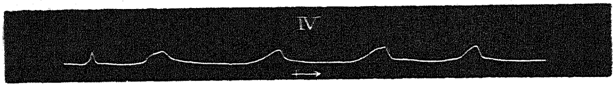
Normal respiration.



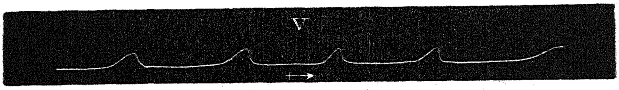
Slightly exaggerated breathing.



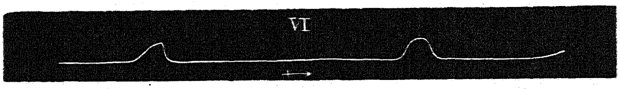
Commencing slowing.



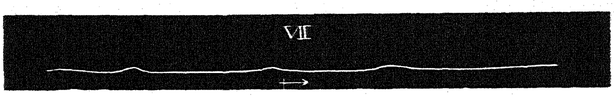
Slowing.



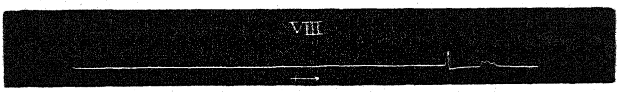
Slowing greater. Sudden and abrupt inspiration and expiration.



Great slowness.

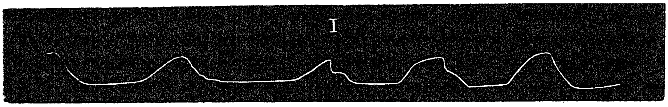


Respiratory movement barely perceptible.

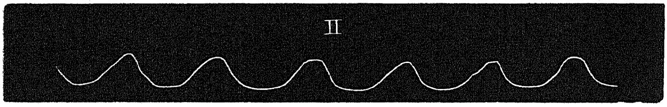


Convulsions commencing.

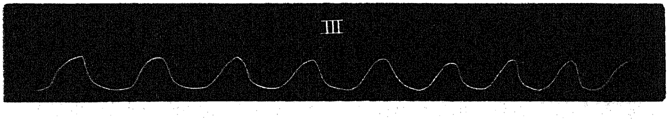
No. 2.—Respiratory Tracings from a Fowl that died very rapidly from a large dose of Cobra Poison. The tracings are nearly continuous.



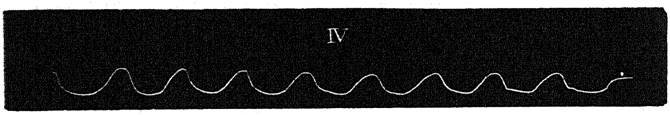
Slow and deep but natural respiration.



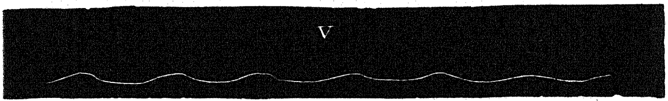
Quickened.



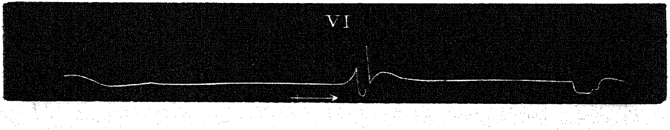
Still more quickened.



Slightly lessened excursus.



Excursus very slight.



Respiration ceased—Convulsions.

ful to allow, of the accelerating action of the poison being perceived. But this quickening, however produced, is merely temporary, and is followed by retardation. The simplest form in which to see the effect of cobra poisoning on respiration is afforded by the common fowl. The stethometric chart, marked No. 1, gives in a concise manner the effects of cobra poisoning when the action is very gradual.

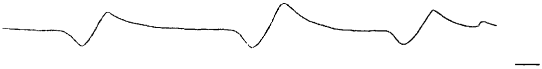
The main points to be noticed are the slight quickening first perceived, and the increase of the excursus. These are followed by rapidly increasing retardation with a certain amount of lessening of the excursus, though the excursus is less affected than the frequency. It is also to be noticed that inspiration becomes sudden and abrupt, and is immediately followed by an expiration equally sudden. The movement that remains is, therefore, peculiarly unfavourable for respiration. In the end the respiratory movement is entirely abolished, and after a pause the convulsions of asphyxia terminate life. Chart No. 2 presents some contrasts of interest. It gives nearly the whole course of the respiratory movements of a fowl from the injection of the poison till death, in a case in which a large amount of cobra poison was given, causing death very rapidly. From it will be seen how very much more pronounced the acceleration is when a large quantity of poison is given, and that when the stage of acceleration is passed the excursus is lessened quite as rapidly as the frequency. The respirations before the administration of the poison bear to the respirations at their greatest degree of acceleration in the tracing IV a ratio of 4 to 7. Chart No. 3 is a series of tracings from a large pariah dog, the acceleration followed by retardation, and the accompanying diminution of excursus are well shown, but the chief points of interest are in the tracings of the occurrence of convulsions; they begin by regular contractions of the inspiratory muscles, in the period of their greatest violence they lose all respiratory character, and then gradually fade away in gentler and gentler attempts at inspiration. The cat is an animal that shows a peculiar power of resisting cobra poison, presenting a marked contrast to the dog. Chart No. 4 is from a cat in whom this resisting power was well shown. After the retardation of respiration is accomplished, it will be noticed that an occasional deep respiration occurred; it is as if the animal, aware of its lessening breathing power, made conscious efforts to assist respiration. A similar feature appears to have been frequently noticed in the human subject under like circumstances. The series of tracings in Chart No. 5 shows graphically the instantly destructive character of cobra poison on the respiratory function. They were taken from a dog on whom the following observations were made.

Experiment V.

A powerful pariah dog had 1 cub. centim. of fresh cobra poison

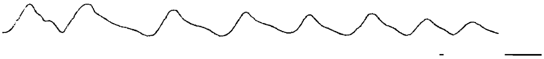
No. 3.—Tracings of Respiratory Movements of a Dog with Cobra Poisoning.

I



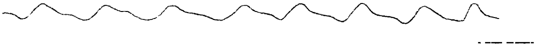
Normal but deep respiration.

II



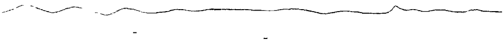
Quickened.

III



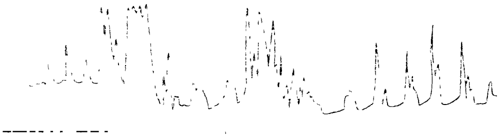
Lessened excursus.

IV



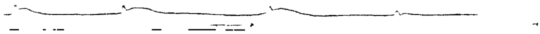
Movement greatly lessened.

V



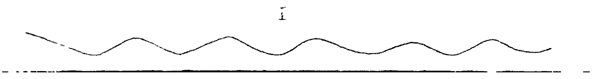
Convulsions.

VI

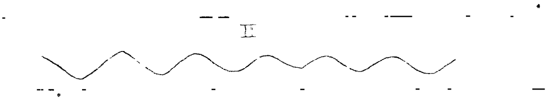


Termination of convulsions.

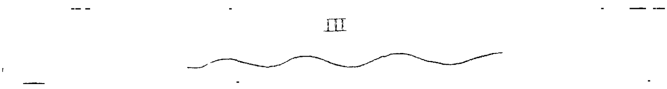
No. 4.—Tracings of the Respiratory Movements of a Cat under the Influence of Cobra Poison.



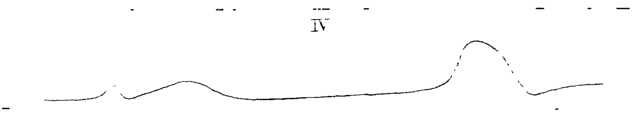
Normal respiration.



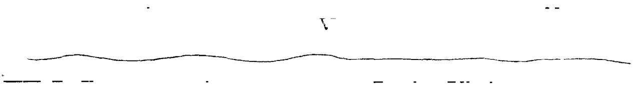
Quickened by cobra poison.



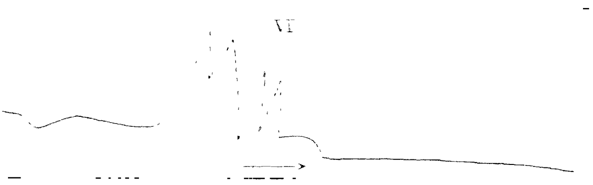
Commencing slowing.



Occasional deep inspiration.



Respiration barely perceptible.



Convulsions.

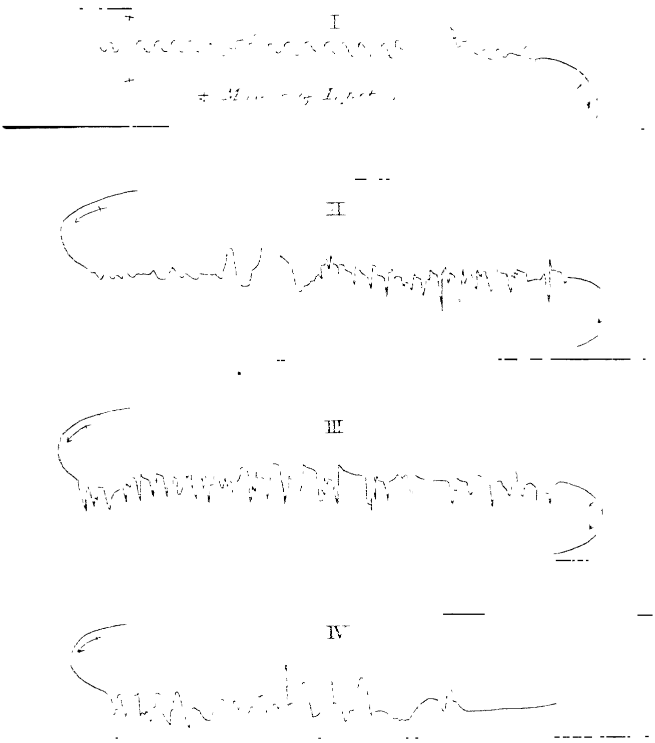
injected into its saphena vein. No change was noticed for thirty seconds, at the end of that time normal respiration abruptly stopped, its place being taken by violent and irregular contractions of the respiratory muscles. The heart's action continued strong, but increased greatly in frequency. Very soon all movement ceased, with the exception of that of the heart, which continued acting for about ten seconds longer. The whole time from injection to death was under 100 seconds.

Chart No. 5 is a continuous tracing of the respiration of this animal from the administration of the poison till death. It will be seen that the normal respiratory rhythm is suddenly displaced by violent alternate upheavings and depressions of the chest walls, and that then all movement ceases suddenly. But it will be noticed that the convulsions, though of course irregular, have a distinct respiratory character. When cobra poison enters the blood rapidly and in large quantity, its first action is to stimulate the respiratory centre so as to cause more irregular respiratory movements, and then one might almost say simultaneously it paralyses it completely, the heart acting for a short time longer. This, however, is not the usual course of events. A much more common method is the one depicted in the stethometric charts, Nos. 1, 2, and 3, where a fair amount of cobra poison is gradually absorbed, as from the bite of a cobra, and where there is in consequence gentle primary acceleration of respiration, with gradual lessening of its rapidity and excursus, and death with convulsions. In still more gradual cases, the primary acceleration is very faint, and the diminution of the respiratory function very gradual, and there is often an attempt—half conscious—to fight against the poison by occasional deep inspirations; at last respiration is arrested, and generally without convulsions, the heart stops after a short interval. This appears to be the usual course in man.

On the circulation, cobra poison cannot be said to exercise a very great influence. The heart nearly always acts for some little time after respiration has ceased, and if the place of normal respiration be supplied by artificial means, the heart will continue acting for very many hours. On the temperature, also, no decided effect can be determined. There may be a slight rise or even fall, but in several cases, watched carefully for the purpose, no change was observed. Nor is there any noticeable effect on the special senses, the poisoned animal appears to hear and see perfectly. The pupil of the eye is also unaffected, and answers to light. If life be preserved by artificial respiration, the pupil remains somewhat contracted. I once noticed the pupil remain contracted after death from cobra poisoning, in which long continued artificial respiration had been resorted to.

On secretion, generally, cobra poison has great influence; nearly all secreting tissues are stimulated by it. The lachrymal glands act

No. 5.—Continuous Tracing of Respiratory Movements of Dog from the moment of Intravenous Injection of Cobra Poison till Death.



freely, and salivation is a most marked and constant symptom. It is rarely absent either in men or in dogs. Saliva often runs in streams from the mouth. The whole of the mucous tract is also in an active state of secretion. After the stomach has been thoroughly emptied by vomiting, the animal will often bring up repeatedly large quantities of mucus, and mucous discharges are also frequently evacuated from the rectum. The respiratory mucous membrane is also similarly affected. Little can be known of its action on the liver or kidneys, the time of observation being so short; in the more chronic cases the kidneys often act freely, but sometimes there is a diminution of urine.

There is but little evidence of the effect of cobra poisoning on the blood. The blood is clearly the carrier of the poison to the system, and it is necessary for it to be some short time present in the blood before a physiological effect is produced, except in those rare cases in which an overwhelming quantity of poison is injected into a vein directly. In man, after cobra poisoning, the blood is nearly always found incoagulable, though in animals, especially dogs, the blood generally coagulates as usual. That there is no great change in the blood is evident from the fact that, when an animal has survived the nerve symptoms produced by cobra poison, it is found to be quite well and to suffer no further inconvenience from blood poisoning or other causes.

Experiment VI.

A large pariah dog had 5 mgrms. of dried cobra poison dissolved in water injected into its hind leg.

12.12. P.M. Injection.

4 P.M. Vomited.

7.30 P.M. Salivated. Looks depressed.

10.15 P.M. Still depressed; salivation slight; respirations 14.

12.5 A.M. Still salivated; very depressed.

8 A.M. Very weak, hardly able to walk; all the legs equally affected.

8.25 A.M. Respirations 12; pupils somewhat dilated, but contract to light. Site of injection very red, hot, and swollen.

11.30 A.M. Respirations 15; frothy salivation.

12 P.M. Rectal temperature, 39° C.

12.38 P.M. Pulse about 120, extremely irregular; chewing movement of jaw and lips; salivation considerable.

2.21 P.M. Better, can walk, salivation ceased; ate sparingly.

7.30 P.M. Recovering fast. Urinated—no albumen in the urine.

8 A.M. Seems quite well. Purulent discharge from the site of the injection; respirations 20, pulse 120.

The dog was kept under observation but remained quite well. The normal pulse rate was 90 and respirations 28.

Here, though the most severe nerve symptoms were present, when they passed off the animal was quite well, and suffered no further inconvenience. The same also occurs in the human subject. Dr. Vincent Richards relates a case of cobra poisoning, from his own observation, in which a man lost completely all power over his legs, was unable to speak, to move the lips, or to swallow, and where there was profuse salivation, and yet after a few hours complete recovery ensued, the man by the next day being well. The microscope also gives no evidence of structural change in the blood. In cobra poisoning, also, albumen in the urine is unknown. In animals that have suffered most severely from nerve symptoms in which I have tested the urine, albumen has not been present in a single case either fatal or non-fatal. When, however, artificial respiration has been performed for some time after apparent death, it is not unusual for blood to be present in the urine; but if the kidneys are examined in these cases the wonder will be, not that blood was found in the urine, but that any urine was secreted at all, so great is the renal congestion. This circumstance can, therefore, hardly be taken as evidence of blood change and coupled with the fact of the rapid recovery that ensues in cobra poisoning after the nerve symptoms have passed off, leads us to the conclusion that all cases of death from cobra poisoning have their origin in the direct action of the poison on the nerve-centres.

II.—*The Physiological Effects of the Poison of the* Daboia Russellii.

The *Daboia Russellii,* or Russell's viper, is selected as being an extremely deadly member of the viperine family, and is the best known of the Indian vipers. The following experiments show the course of the symptoms produced by this snake.

Experiment VII.

A somewhat small pariah dog had a pulse of 88 per minute, respirations 30, rectal temperature 39° C.

3.24 P.M. Bitten by a *Daboia Russellii* in the thigh.

3.28 P.M. General muscular spasm of the most violent character, all parts of the body taking part. The animal fell down and rolled about in convulsions, even the muscles of the eyeballs being affected —jerking the eyes about in the strangest way.

3.30 P.M. Convulsions ceased, complete paralysis of both hindlegs, tries to stand, but can only rest on his forelegs.

3.31½ P.M. Contractions of muscles of eyeball continue; respirations very shallow, 68; pupils somewhat dilated.

3.37 P.M. Muscles of eyeball at rest; seems utterly prostrate and unable to move; respirations 40, chiefly abdominal.

3.40 P.M. Respirations 56, shallow, occasionally a deep sigh.

3.44 P.M. Pupils somewhat dilated but contract to light.

3.47 P.M. Respirations 67, about every tenth one is very deep.

3.53 P.M. Pulse 156.

3.56 P.M. Moaning. Temperature 39°·5 C.

4.1 P.M. Respirations 32; sighing and moaning, lying down paralysed, sanious discharge from the rectum, pupils widely dilated.

4.9 P.M. Respirations 36.

4.12 P.M. Respiration reduced to a quick inspiratory spasm, followed by relaxation.

4.15 P.M. Respirations 16.

4.17 P.M. Dead. Temperature 39°·4 C.

Experiment VIII.

A small quantity of fresh daboia poison (about 0·1 cub. centim.) was injected into the thigh of a pariah dog.

12.20 P.M. Injection.

12.25 P.M. Very lame in the leg in which the injection was made.

12.52 P.M. Very drowsy, unsteady in walking.

12.54 P.M. Pupils widely dilated; iris only just visible.

12.56 P.M. Moaning.

1 P.M. Panting; raised himself to a standing posture, but his legs gave way under him, and he fell to the ground.

1.5 P.M. Violent respiratory movements; respirations 28.

1.6 P.M. Still able to move his head freely.

1.15 P.M. Tries occasionally to rise.

1.17 P.M. Moaning.

1.20 P.M. Moderately loud screams.

1.25 P.M. Convulsions.

1.34 P.M. Dead.

Experiment IX.

A pariah dog had 5 mgrms. of dried daboia poison injected subcutaneously, dissolved in 1 cub. centim. of water.

12.56 P.M. Injection.

1.54 P.M. Quite well. Respirations 44.

7 P.M. Ate freely.

6 A.M. Looks a good deal depressed. Respirations 48 a minute.

7.20 A.M. Pupils contracted, panting; respirations 60 a minute.

8.15 A.M. Panting excessively.

8.30 A.M. Passed a sanious discharge per rectum.

8.45 A.M. Respirations 80, excessive dyspnœa, blood oozing from mouth.

9.30 A.M. Dead.

After death the fatal dyspnœa was found to be dependent on œdema of the lungs.

Experiment X.

A small pariah dog was bitten in the thigh by a small *Daboia Russellii.*

12.34 P.M. Bitten.

12.35 P.M. Slightly panting.

12.35½ P.M. Fell over suddenly in violent convulsions, the hind-legs being especially strongly convulsed, pupils contracted.

12.36 P.M. Slight attempt at respiration.

12·39 P.M. Dead.

From these experiments it will be seen that there is a great diversity in the symptoms between different cases of daboia poisoning. The animal may fall down at once in convulsions and expire as in Experiment X, or it may never have any nerve symptoms at all, but die many hours after from a remote affection as in Experiment IX.

The following experiments throw light on the causes of these differences.

Experiment XI.

A pariah pup had about 0·1 cub. centim. of fresh daboia poison injected subcutaneously into the shoulder.

12.52 P.M. Injection.

1 P.M. Vomiting.

1.4 P.M. Can stand but cannot walk.

1.8 P.M. Cannot stand; moaning.

1.11 P.M. Slight sanious discharge from the rectum.

1.15 P.M. Short rapid respiration with an occasional deep inspiration.

1.30 P.M. Respiration very slight.

2.6 P.M. Dead.

Experiment XII.

A pariah pup of the same litter as the one in the last experiment, and as nearly as possible of the same size, had 0·3 cub. centim. of fresh daboia poison injected into its shoulder subcutaneously.

1.31 P.M. Injection.

1.35 P.M. Fell over in convulsions.

1.37 P.M. Unable to stand.

1.45 P.M. Lying down, is quite paralysed, but occasionally groans.

2.1 P.M. Totally unconscious.

2.16 P.M. Respiration failing.

2.29 P.M. Dead.

In these two dogs, which were of the same age and size, the daboia poison was injected into exactly the corresponding part of each, and subcutaneously; but, in the latter experiment, three times as much poison was injected as in the former case. This was the only difference in the two experiments, and the result was that the animal which had the larger quantity of poison suffered in four minutes from violent convulsions, and from that time was quite paralysed; whilst the other only gradually became paralysed, and had no sign of convulsions at all. The poison in both cases was from the same viper, and extracted at the same time. In the same way it can be proved that an equal quantity of poison injected into two animals of different sizes will kill the small one almost instantly in convulsions, but will only cause death after a long interval, and without violent symptoms, in the larger one. Daboia poison, therefore, commences its constitutional action by causing convulsions, though a certain proportion of animals will escape them through an insufficient quantity of poison having been injected. It should be stated here, that the preliminary effects of the poison differ only in degree from those of cobra poisoning. The local pain appears to be peculiarly acute, and the accompanying inflammation exceedingly severe; and, from the amount of blood-stained serum effused, might almost be said to have a "specific" character.

The contrast between the effects of the two poisons comes out singularly clearly when birds are selected as the subject of experiment.

Experiment XIII.

1 cgrm. of dried daboia poison was dissolved in 1 cub. centim. of distilled water, and the solution was injected with great care just beneath the skin of the leg of a fowl.

3.10 P.M. Injection.

3.11 P.M. Slightly lame in the leg in which the injection was made.

3.12 P.M. Violent convulsions.

3.12¾ P.M. Dead.

Experiment XIV.

1 cgrm. of dried cobra poison dissolved in 1 cub. centim. of distilled water was injected just beneath the skin of a fowl.

3.30 P.M. Injection.

3.34 P.M. Drowsy.

3.36 P.M. Beak resting on the ground; unable to stand; eyelids closed, pupils contracted.

3.37 P.M. Unable to lift its head from the ground.

3.40 P.M. Convulsions.

3.42 P.M. Dead.

In these two experiments the greatest care was taken to ensure the conditions being exactly the same; yet, with daboia poison, the bird died in two and three-quarter minutes with only the occurrence of convulsions; while, with cobra poison, it died after twelve minutes, and went through the regular course of paralysis. The convulsions, therefore, are not dependent upon the injection of a large quantity of poison directly into a vein. The next point to be determined is whether the convulsions are due to asphyxia, like those that often occur after the cessation of respiration from paralysis in cobra poisoning.

Experiment XV.

A fowl had its trachea opened, and a tube in connexion with a bellows for artificial respiration inserted into it. 2 cgrms. of dried daboia poison in solution were then injected subcutaneously into its leg. Immediately on the injection being completed, artificial respiration was commenced.

2.23 P.M. Injection.
2.23¼ P.M. Artificial respiration commenced.
2.26 P.M. Convulsions, in which the bird died.

Here, though ample means were employed to keep the blood oxygenated, the bird died at once in convulsions. The convulsions of asphyxia that occur after the paralysis of cobra poisoning are removed by artificial respiration. It is clear, therefore, that the convulsions of daboia poisoning are *primary*, and in no way due to defective aëration of the blood.

The convulsions are exceedingly violent in character, the whole of the voluntary muscular system being affected, even to the muscles of the eyeball. Sometimes, but rarely, the convulsion is tonic, the muscles clasping, as it were, the frame of the victim. The convulsions may be due either to the direct action of the poison on the muscles, or on the muscular terminations of the nerves, or on the central nervous ganglia. The following experiment determines this.

Experiment XVI.

A fowl was placed under the influence of chloroform, and its right crural nerve divided; the left crural nerve was then isolated, and a ligature placed round all the other structures so as completely to obstruct the circulation, but the nerve was left uninjured. The moment the bird became conscious, a small quantity of solution of daboia poison was subcutaneously injected into the tissues at the back of the neck. Almost directly after, convulsions occurred, but, in these convulsions, the muscles supplied by the right or divided nerve took no part, remaining perfectly flaccid, while the muscles of the left leg were violently tetanized. Here the muscles of the right leg which were freely supplied with the poisoned blood, but the nerve to which was divided, escaped the convulsions; while those of the left leg, which had no contact with the poisoned blood, but whose nerve was still in communication with the central nervous system, took part in them. It is evident, therefore, that the convulsions depend upon the direct irritation of the central nervous system by the poison, and in no way on the contact of the poison with the muscles or with the terminations of the nerves.

These *primary* convulsions occur in from one to ten minutes after the infliction of the bite, and they may have any degree of severity, from those producing almost instant death to merely a few muscular twitchings. The course of the symptoms after the convulsions also varies. Respiration may never be thoroughly re-established, the animal dying very soon, or respiration may recommence, but complete general paralysis is found to have supervened, and the animal dies of consequent asphyxia. It may happen that, for a short time, the victim recovers completely from the convulsions, walking about for a few minutes as if nothing had occurred, but the fatal paralysis is only delayed for a short time.

It appears as if daboia poison acted somewhat differently in different classes of the animal kingdom. Birds are most easily affected, and next to them come the Lacertilia.

Experiment XVII.

About 0·4 cub. centim. of fresh daboia was injected beneath the skin of the upper surface of the tail of a large specimen of *Calotes versicolor*.

The respirations were 54 per minute.

3.49 P.M. Injection.
3.51 P.M. Convulsions.
3.52 P.M. Quite paralysed.
3.53 P.M. Respirations 12 per minute.
3.56 P.M. Respirations 4 per minute.
3.59 P.M. Dead.

Mammals also are very easily affected by the convulsion-producing property of the poison, whereas the Amphibia only have symptoms of gradual paralysis.

Experiment XVIII.

A medium sized frog (*Rana tigrina*) had 1 cgrm. of dried daboia poison in solution injected into its dorsal sac.

1.12 P.M. Injection.
4.30 P.M. Violent respiratory efforts.
4.39 P.M. Fast becoming paralysed.
4.53 P.M. Completely paralysed.
5.11 P.M. Dead.

Though birds are so sensitive to the agent in the poison that produces these nervous discharges, that it is difficult to poison them at all without the occurrence of primary convulsions—1 mgrm. often producing them—yet by heating daboia poison in solution to 100° C. it loses completely the power of causing this symptom even in them.

Experiment XIX.

5 cgrms. of dried daboia poison in solution were heated for a short time to 100° C. The solution was then injected subcutaneously into the leg of a fowl.

12.27 P.M. Injection.
12.34 P.M. Seems drowsy; eyes closed.

12.45 P.M. Breathing rapid.
12.47 P.M. Respiration failing, comb becoming purple.
12.49 P.M. Lying down, head drooping.
12.50 P.M. Fallen over on its side; respiration fainter.
12.53 P.M. Gasping.
12.56 P.M. Convulsions.
12.58 P.M. Convulsions lessening.
12.59 P.M. Dead.

Here, though the amount of poison injected was more than fifteen times that required to cause almost instant death from convulsions, simple exposure to a heat of 100° C. completely altered the symptoms. They were those of gradual paralysis which caused the respirations to cease, and the convulsions which occurred at the end of the paralysis were simply the expression of carbonic acid poisoning. They were secondary, and no longer primary. Even with the abolition of primary convulsions, however, the symptoms differed materially from those of cobra poisoning.

Should primary convulsions not occur the history is that of advancing paralysis. The respirations and pulse become greatly accelerated, and there is gradual loss of power in all the limbs; vomiting may occur. Sanious discharges issue from the rectum and other parts. The pupils are usually widely dilated, and the respirations become less and less, and may cease with or without convulsions.

But there is a third form of death from daboia poisoning, quite unlike anything that is seen in cobra poisoning. It occurs in those cases in which but a very small quantity of poison indeed has been injected. The animal has very few nervous symptoms, very likely none at all; but on the second day he appears ill, refuses food, has diarrhœa, his urine contains albumen, and he may linger on in this state for days, dying exhausted, or some acute complication may supervene as in Experiment IX, causing death rapidly. In that case it was an œdematous condition of the lung that proved fatal, whereas in a case recorded in the "Indian Medical Gazette" (June 1, 1872), it was a hæmorrhagic condition of the system generally. The snake was evidently from its description the *Daboia Russellii*, or its congener the *Echis carinata.*

A Mahomedan, forty years of age, was bitten by a snake on the finger. The bite was soon after incised, and stimulants given. The hand and arm became much swollen, and on the same day he passed blood by the rectum, with his urine, and he also vomited blood. The next day he was sick, and was still passing blood from all the channels. In this state he remained eight days, losing blood constantly, and dying in consequence exhausted on the ninth day.

In this case no symptoms occurred for many hours, but when they did supervene they increased in severity till the man died from hæmorrhage. There was no paralysis or any other special symptom. The case presents in every detail a complete contrast to Dr. Richard's case of cobra poisoning, where for some hours the patient was in the greatest danger from nerve symptoms, and yet the next day made a full and complete recovery.

This chronic form of daboia poisoning occurs whenever only a small quantity of daboia poison is injected.

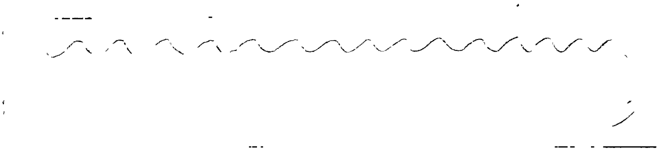
Experiment XX.

A cat had about 5 mgrms. of daboia poison in solution injected into its leg. No symptom was noticed for about twelve hours, when it became ill, refused food, had diarrhœa, and remained in this state till the fourth day, when it died.

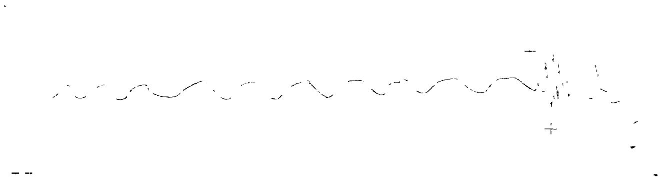
In regard to the paralysis caused by daboia poison, there a few points to be observed. It certainly supervenes earlier than in cobra poisoning, but it does not extinguish the respiratory function nearly so soon. Paraplegia is occasionally noticed in dogs, especially if the hind legs have been much convulsed. It is probably due to exhaustion of the conducting elements of the cord by the violence of the convulsion-producing discharges. Care, however, should be taken not to confound it with inability to use the hind quarters occasioned by the pain of the local injection, which appears to be peculiarly severe. Again, the paralysis of the lips, tongue, larynx, and pharynx, which is so marked a feature in cobra poisoning, is absent in daboia poisoning. The tongue, instead of being pendulous, is retracted, and the larynx, so far from being paralysed, gives utterance to loud screams, often as long as life lasts, as in Experiment VIII. It is as if the poison exerted its paralysing influence on the main motor tract, and had not that marked affinity for the respiratory and allied centres that cobra poison has; a conclusion borne out by the way in which it commences its action, and by the time it takes to extinguish the respiratory function.

The course of the respiration in daboia poisoning is of necessity as varied as there are modes of death from the poison. Chart No. 6, from a fowl, gives a typical example of respiration when the primary convulsions are fatal. It contains nearly all the chest movements from the moment of injection till death. The first sixteen respirations are normal, the excursus is then slightly increased, and a slight retardation is to be observed; then without further warning, the most violent convulsions took place, and with them life was extinguished. It affords corroborative evidence of the fact, that asphyxia has nothing to do with the causation of the convulsions, as respiration is perfectly well performed up to the moment of their occurrence. Chart No. 7 is from a fowl that was poisoned by daboia poison, which had been heated in solution to 100° C., and so deprived of its power of causing

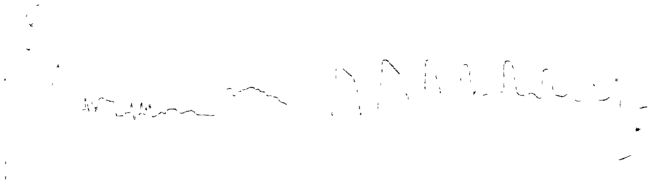
No. 6.—Tracing of Respiratory Movements in Fowl under the Influence of Daboia Poison.



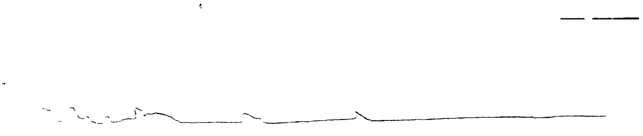
Respiration after the injection of the poison, but still normal.



Slightly exaggerated breathing, suddenly interrupted by convulsions.



Convulsions.

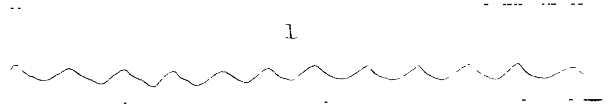


End of convulsions.

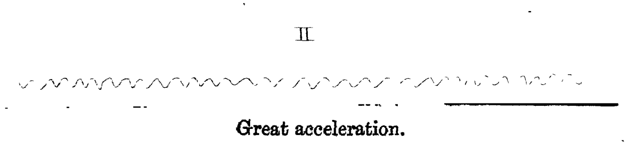
convulsions. The acceleration of the respiratory movements is most remarkable, as is also the way in which the excursus is exaggerated and maintained; it is only at the very end of life that the respiratory movement is diminished.

Chart No. 8 is from a dog, to which sufficient poison was not given to cause convulsions. The very much greater acceleration of the

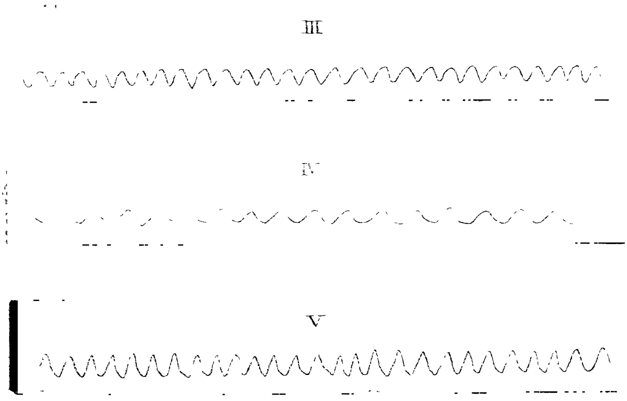
No. 7.—Tracing of Respiratory Movement in a Fowl under the Influence of Daboia Poison that had been heated to 100° C.



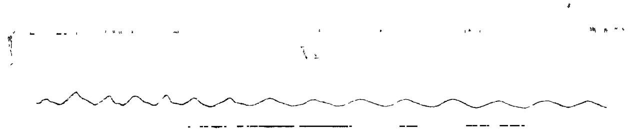
Normal.



Great acceleration.



Great acceleration with increased excursus.

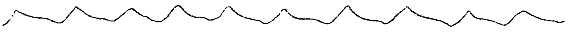


Lessened excursus.

respiration than can be caused by cobra poisoning is seen, the respirations in I and III being as two to five. But in IV, a singular condition is to be noticed, that is exceedingly common in daboia poisoning. It is a peculiarly deep inspiration, followed immediately by expiration. It occurs generally once in every ten or fifteen respirations, and at a

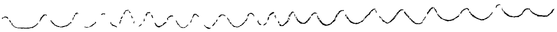
No. 8.—Tracings of Respiratory Movements of Dog under Daboia Poisoning.

I



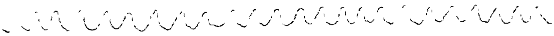
Normal respiration.

II



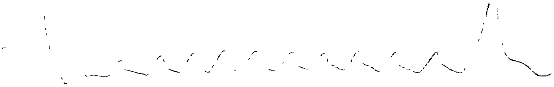
Acceleration.

III



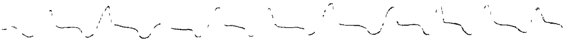
Respiration still quicker.

IV



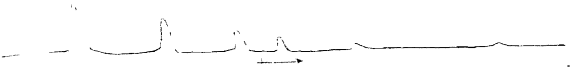
Irregular respiration.

V



Respiration slowing.

VI



End of respiratory movements.

time when the animal is often profoundly unconscious. It is quite characteristic of daboia poisoning, though it is not always present, sometimes the whole body taking part in the movement. After the primary convulsions have occurred, it is exceedingly rare for death to be preceded by the convulsions of asphyxia, the respiratory function appearing gradually to fade away, as in the tracing VI.

In its influence on the temperature and circulation, daboia poison does not differ materially from cobra poison.. But while cobra poison has no effect on the pupil of the eye, wide dilatation is always, or nearly always, to be seen during some stage of daboia poisoning. The iris is sometimes barely visible during the primary convulsions. Over secretion, daboia poison has also some power, though less than cobra poison, and the mucous discharges are nearly always largely mixed with blood. But salivation, the constant accompaniment of cobra poisoning, is almost unknown in daboia poisoning. Out of a large number of experiments, I have only seen it once, and in this case it was by no means marked, and might easily have been overlooked, unless special attention had been paid to its occurrence.

In daboia poisoning there is a good deal of evidence to be considered as to the effect of the poison on the blood. It is almost universally found after death, that the blood has been rendered uncoagulable, the only exceptions being when the animal has died almost instantly in convulsions, or in those cases where the animal has struggled for a long time against the poison, when the coagulation will be found to be imperfect, as if an attempt towards restoring the normal condition of the blood had been made. Even in rapid cases of daboia poisoning, we have proof that the relation of the blood to the tissues is altered. In Experiment VII a sanious discharge occurred thirty-seven minutes after the infliction of the bite. So grave are these changes in the blood, that they are by far the most frequent causes of death in daboia poisoning (Experiments IX and XX). The hæmorrhages chiefly take place from the rectum and kidneys, but I have seen them from the mouth, and even the skin.

In cobra poisoning albuminuria is unknown, but in every case of daboia poisoning in which symptoms were present, and in which six hours elapsed before death, I have detected albumen in the urine. It is generally not in large quantity, but it is quite unmistakeable.

Experiment XXI.

A solution of about 5 mgrms. of daboia poison was injected into a dog.

4 P.M. Injection.

8 A.M. No symptom.

4 P.M. 5 mgrms. more of daboia poison injected.

9.30 P.M. Affected; respirations 50; pupils widely dilated.

6 A.M. Depressed and ill.
3 P.M. Passed some albuminous urine.
9 P.M. Diarrhœa.
12 P.M. Urine albuminous; still very ill.

As it was clear that the animal would suffer the usual prolonged course of blood-poisoning, more poison was injected to shorten life.

1.30 P.M. Injection.
3.17 P.M. Cannot stand.
3.20 P.M. Respiration failing.
3.29 P.M. Dead.

There is evidence, therefore, that daboia venom is a most severe blood poison, producing death when not administered in sufficient quantities to cause serious nerve symptoms, and that it is even possible for death to occur early from this cause alone, nerve symptoms not having supervened.

Conclusions.—The regular course of the symptoms of cobra poisoning is slowly advancing general paralysis, coming on after a well-marked interval without symptoms, with special paralysis of the lips, tongue, larynx, and pharynx, and complete destruction of the respiratory function. Death is often attended by convulsions, which depend on asphyxia.

Daboia poison commences its action by inducing violent general convulsions, which are often at once fatal, or may be followed immediately by paralysis and death, or may also be for a short time recovered from, paralysis and death following later. These convulsions do not depend on asphyxia, and they may be absent if only a small quantity of poison has been injected. The paralysis that succeeds is general, and lasts a considerable time before respiration is extinguished, and there is no special paralysis of the lips, tongue, larynx, and pharynx.

Cobra poison very quickly destroys respiration; after slight acceleration the respiration becomes slower, and the excursus is lessened.

Daboia poison at first quickens the respiration very much more than cobra poison does, and the lessening of the excursus, and the retardation of the respiratory movements do not occur so soon.

Daboia poison invariably kills birds and reptiles at once in convulsions; cobra poisoning only after paralysis.

The effect of cobra poison on the pupil is so small as to be a matter of doubt; daboia poison causes wide dilatation of the pupil. Salivation is a constant symptom in cobra poisoning; it is exceedingly rare in daboia poisoning.

The effect of cobra poison on the blood is not great, sanious discharges are rare, albuminuria has not been seen, and should the patient not die from the paralysis, recovery is perfect and complete, no symptom being left in a few hours.

In daboia poisoning, sanious discharges are the rule; albuminuria is always found, should the victim live any time, and after the nerve symptoms pass over, the subject has to go through a period of blood poisoning little, if at all, less dangerous than the primary symptoms, from which he may die as late as the end of the second week.

Lastly, the physiological properties of daboia poison undergo great change by its being heated to 100° C. in solution, whereas cobra poison remains unaltered.

II. "On Pendent Drops." By A. M. WORTHINGTON, M.A. Communicated by Professor B. STEWART, F.R.S. Received May 16, 1881.

[PLATE 7.]

About two years ago I was led to examine the forms of pendent drops of liquid by a method of great simplicity, which seems capable of being used with considerable accuracy for determining the value of the surface tension.

Previous observers, so far as I am aware, have observed only the weight of drops which fall, and, making this the basis of calculation, have endeavoured to find the influence on the size of such drops, of the rate of influx of liquid, shape of terminal, as in the case of Dr. F. Guthrie,* or to ascertain the value of the surface tension of the liquid as in the case of Professor Quincke† and M. Dupré.‡

Under no circumstances, however, is the weight of the drop which falls exactly the weight of the volume which it is necessary to know in order to ascertain the value of the surface tension, though under certain circumstances it approximates thereto. Hence we find that Prof. Quincke rejects this method of finding the tension, or recommends it only where other methods fail;§ albeit all his results obtained by this method are vitiated by an assumption to which I shall have occasion to draw attention. The principle of the method which I will now describe is simply to project a magnified image of a drop pendent from a cylindrical tube on to a screen, and there to trace its outline at any required stage of its development.

A vertical cylindrical glass tube, A, whose lower end is ground truly flat and with a sharp edge, communicates by means of a bulb or wider tube, B, and a piece of india-rubber or lead tubing, C, with an air-tight syringe, D, some 10 or 12 feet away. For the usual syringe piston a cup of mercury, E, is substituted, which can be gradually raised or lowered by a rack and pinion-screw, so as to serve as a perfectly air-tight piston.

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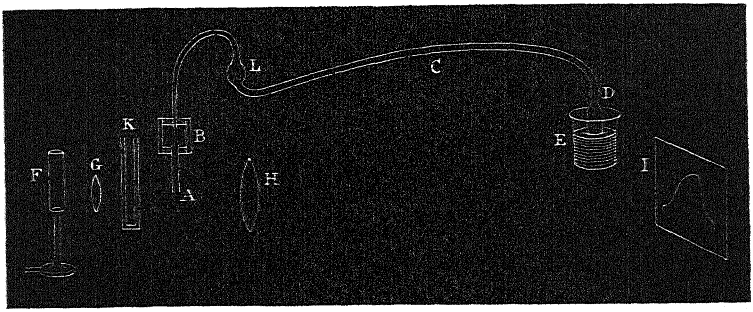
* "On Drops." By Dr. F. Guthrie. "Proc. Roy. Soc.," vol. 13, p. 444.

† "Poggendorff's Annalen," vol. cxxxv, p. 621. "Phil. Mag.," 1869.

‡ "Théorie Mécanique de la Chaleur," p. 332.

§ *Loc. cit.*, p. 637, § 12.

Fig. 1.



By means of an Argand lamp, F, whose light is condensed by the lens G, a magnified image of the end of the tube is thrown by the lens H, upon a piece of white paper, I, fixed on the wall of the room near the syringe.

K is a plate-glass cell filled with water to keep off the heat of the lamp.

A supply of the liquid to be observed is drawn up into the bulb or wide tube B, by means of the syringe, or more conveniently by means of an india-rubber bulb, L, interposed for the purpose between B and D.

The tube A is then carefully wiped on the outside with a clean linen cloth, and the observer standing by the paper gradually expels the liquid by raising the mercury cup, and a drop is extruded which wets the whole of the bottom of the tube, the outline of which he traces on the white paper with a finely-pointed lead pencil, at any stage of which he desires a record, up to the point of rupture.

This tracing affords all the information necessary for calculating the value of the surface tension, the amount of magnification being known from the ratio of the diameter of the tube to that of its image on the screen.

As the first few drops expelled are apt not to wet the whole of the base of the tube thoroughly, and therefore to be rather smaller than those which follow, it is well to let one or two fall off before taking a tracing if a series of outlines at different stages is required.

The tube A can be exchanged at pleasure for others of different diameters, and when it is wished to observe the form of a drop of one liquid pendent in another, the latter is placed in a plate-glass cell with parallel sides, and the tube A dips into it.

The figures of the Plate are tracings (reduced one-half) obtained in this way with different liquids and tubes of various diameters, and show the manner in which the form of the surface alters with the growth of the drop.

The instability of the figure close to the point of rupture makes it difficult to trace the outline correctly quite to that point.

The short horizontal line below the drop shows the maximum depth attained before rupture, and the area enclosed by the last outline that could be drawn correctly, and the one which is nearest the limit of stability is shaded for the sake of distinctness. The dotted outline, generally the innermost, shows the amount of liquid which remained adhering to the tube after the separation of the lower portion.

It may be worth while to mention here that where the liquid wets the whole of the base of the tube the form and dimensions of the outline were found to depend on the external and not at all on the internal diameter of the tube.

One of the first points that strikes anyone inspecting these figures is, that the maximum depth attained varies surprisingly little with the diameter of the tube.

An early series of observations on this point made with insufficient magnification showed so slight a variation that I was led to suppose that the maximum depth might really be constant and independent of the size of the tube, and to seek for an empirical formula involving only the physical constants of the liquid to express this depth.*

Greater magnification, however, shows that this is not the case; that the maximum depth diminishes on the whole with the size of the tube; but is rather less in a drop pendent from a plane surface of indefinite extent than in one hanging from a tube just narrow enough for the drop to attach itself at the edge.

Another point that attracts immediate attention is, that whereas the form of drops of different liquids hanging from the same tubes differs materially (compare, for example, water and turpentine), yet drops of (say) turpentine hanging from narrow tubes are similar in form to water drops hanging from wider tubes.

That this would be the case has been already shown from theoretical considerations by M. Dupré,† who has also pointed out the ratio between the diameters of the tubes that corresponds to complete similarity of the drop form for two different liquids.

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* A very close approximation is given by the formula—

$$\text{Maximum depth} = \tfrac{3}{4}\pi\sqrt{\frac{\text{tension}}{\text{density}}},$$

which was suggested by the equation to the generating curve.

† "Théorie Mécanique de la Chaleur," p. 329.

The following method of finding the ratio of tube-diameters, necessary for the symmetry of drops of different liquids, being rather simpler in form than that given by M. Dupré, may be welcome to some readers.

It will be observed that with wide tubes the inclination which the extreme element of the curve makes with the horizontal base of the tube gradually increases as the drop is expelled, till it separates; but with narrower tubes it passes through a maximum value, which may be greater than 90°, after which it diminishes again.

Professor Quincke has shown that the angle of contact between a given liquid and solid in air is definite and constant. This, however,

Taking the vertex as origin, and the axis of revolution as the axis of y, the equations to the generating curves of the two liquids may be written—

$$T_1\left(\frac{1}{\rho_1}+\frac{1}{\rho'_1}\right)-\frac{2T_1}{R_1}=D_1y_1 \quad . \ . \ . \ . \ . \ . \ . \quad \text{(i)},$$

$$T_2\left(\frac{1}{\rho_2}+\frac{1}{\rho'_2}\right)-\frac{2T_2}{R_2}=D_2y_2 \quad . \ . \ . \ . \ . \ . \ . \quad \text{(ii)},$$

where T_1 and T_2 are the surface tensions.

$\rho_1\rho'_1$ and $\rho_2\rho'_2$ are the principal radii of curvature at any point,

R_1 and R_2 are the radii of curvature at the vertices,

D_1 and D_2 are the densities of the liquids.

If these curves are symmetrical, the ratio between all corresponding lines must be the same, and remembering that ρ'_1 and ρ'_2, the radii of the curvature due to revolution, are the normals drawn from the curve to the axis, and therefore corresponding lines in the plane of the paper, we have—

$$\frac{R_1}{R_2}=\frac{\rho_1}{\rho_2}=\frac{\rho'_1}{\rho'_2}=\frac{y}{y'}=K,$$

whence, writing $R_1=R_2K$, $\rho_1=\rho_2K$, &c., in (i), and dividing by (ii), we have—

$$\frac{T_1\left(\frac{1}{\rho_2K}+\frac{1}{\rho'_2K}-\frac{2}{R_2K}\right)}{T_2\left(\frac{1}{\rho_2}+\frac{1}{\rho'_2}-\frac{2}{R_2}\right)}=\frac{D_1y}{D_2y'},$$

or

$$\frac{T_1}{T_2}\times\frac{1}{K}=\frac{D_1}{D_2}K,$$

whence

$$K^2=\frac{T_1D_2}{T_2D_1},$$

or

$$K=\sqrt{\frac{T_1D_2}{T_2D_1}}.$$

The empirical formula of the note to page 364 contains the experimental verification of this result, and shows in addition that the limit of stability is reached at corresponding stages by drops that develop similarly, or that this limit does not depend on the *absolute* dimensions of the curve. M. Dupré's tacit assumption that this will be the case seems to me hardly legitimate.

M. Dupré makes extremely sagacious use of the relative weight of drops whose forms while hanging are similar, for finding the surface tension.

My observations, however, go to show that the depths of residual liquid are not usually proportional to the other linear dimensions, and that any such use is not certain to lead to results that are more than approximately correct.

is in no way inconsistent with the variation just noticed, for the angle at which the liquid leaves the tube is practically indeterminate at the sharp edge of the glass where the liquid can select and attach itself to some elementary portion inclined at the required angle. But this constancy of the real angle of contact accounts for the fact that if a very narrow tube be used, the drop will spread up it on the outside before separating. As soon as the angle between the extreme element of the curve and the vertical side of the tube is equal to this definite angle of contact the spread will take place.

It will be noticed, especially with wide tubes, that the base of the drop suffers but little change after a certain point.

The concavity, however, of the upper portion is seen to deepen before the liquid separates, but the drop remains suspended after this deepening has begun.

It is very difficult to tell precisely where separation takes place, since it is accomplished generally with great rapidity, but I am inclined to think that it occurs at the place of greatest concavity.

Sometimes the small secondary drop (observed by Dr. Guthrie) is seen to follow the main drop. This was very noticeable in the case of water dropping through petroleum, where separation is effected comparatively slowly. The point at which this small drop is first seen serves, in this case, to determine fairly accurately the place of separation. There can be no doubt that this secondary drop is due to the spontaneous segmentation of the cylindrical neck of liquid, which joins the upper and lower portions up to the last moment before complete separation takes place; and that it is the same phenomenon that was first observed and explained by Mr. Plateau in his experiments on cylinders of mercury.*

It is obvious that if drops are extruded in rapid succession there will be a considerable influx of liquid through this residual neck into the lower portion after it has begun to separate, and that the drops which fall will on this score be larger.

Since, however, the velocity of the inflowing liquid will produce a pressure on the lower portion of the drop, tending to hasten separation, it is to be expected that a definite increase in the rate of influx will produce a greater effect on the size of the drop which falls off when the rate of dropping is slow than when it is fast.

These considerations go far to explain the results of Dr. Guthrie's experiments on the influence of the rate of dropping on the drop size. It is further evident that an influx of liquid, after separation has begun, only complicates the phenomenon, and that it is from a study of the curve, when the influx is zero, that we are most likely to obtain an insight into the manner and cause of separation.

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* "Statique Expérimentale des Liquides."

Perhaps a statement of the physical circumstances under which the drop develops and separates, in the light of the information afforded by the tracings, may not be out of place here.

The cohesion of a liquid, as is well known, manifests itself in a uniform internal compression (A) which may conveniently be regarded as transmitted from the surface, and, in addition, a surface tension (T) which produces a further pressure proportional, at any point of the surface, to the sum of the reciprocals of the principal radii of curvature at that point, so that the total pressure at any point of the surface may be written,

$$A+T\left(\frac{1}{\rho}+\frac{1}{\rho'}\right).$$

The value of the last term may be positive or negative, according to the nature of the curvature, but the total pressure must always be positive, indeed the term A is always relatively very great.

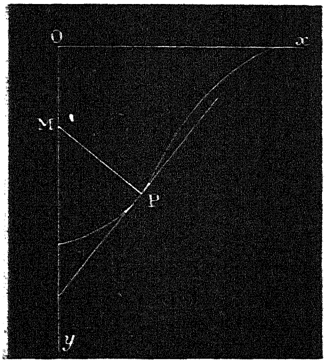
The surface acts in many respects like a membrane stretched uniformly in all directions, and in the case of a pendent drop, the liquid within may be regarded as sustained by the tension of this skin.

On ordinary hydrostatic principles the pressure must be the same at all points in the same horizontal plane, from which condition the equation to the generating curve is at once obtained—

$$T\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)=c+yD,$$

where the axes are those of the figure, D the weight of the unit volume of the liquid, and c a constant expressing the pressure due to curvature at the base of the drop where $y=0$.

FIG. 2.



If we take the radius of curvature in the plane of the paper as ρ, then ρ' is the radius of the curvature due to revolution round the axis of y, and is equal to the normal PM.

It is possible to obtain, by inspection of the curve, some notion of the value of the total curvature as measured by $\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)$ to which the pressure is proportional.

The length of the normal ρ' is of course capable of exact measurement, while that of the radius of curvature ρ may be obtained approximately by striking the circle of curvature.

At a point of recurvature, $\frac{1}{\rho}=0$, and the normal alone determines the pressure.

If the reader will trace in this way, in one of the figures, the value of the total curvature from the vertex of the drop to the base of the tube at different stages of extrusion, and will, in the case of some one liquid, compare the final values attained before separation, when tubes of different diameters are used, he will perceive that, before the drop has attained its maximum size, the tension of the surface *seems* to press the contained liquid against the end of the tube. But, as the drop increases in size, the tension is more and more exerted in supporting the additional weight of liquid, and this pressure at the end of the tube diminishes; and that this pressure at the base, when the drop separates, is greater with small tubes than with wide ones, and is obviously still positive in the case of water hanging from the narrower tubes.

But, with a wide tube, the final value of the total curvature that is attained before rupture is obviously less than with a narrow tube; and, by the methods of measurement, which will be described in the sequel, it was found that, as the diameter of the tube is increased, the final value of the curvature becomes gradually smaller, zero, and then increasingly negative, and here the term A in the expression for the pressure

$$A+T\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)$$

gives meaning to the whole expression which would otherwise be negative and meaningless.

Up to the point at which the curvature at the base is greater than 0 everything would have gone on the same, even though this term A were non-existent,* the tension of the surface sufficing for the support of the drop; but, at this point, the tension ceases to be sufficient, and is aided by the true internal cohesion of layer for layer of the liquid, and the adhesion of each unit of area of the liquid for the solid base with which it is in contact. The extent to which this portion of the cohesion is called into play in sustaining the weight of the drop, is measured by the amount of negative curvature above-mentioned.

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* This is only mathematically, not physically, conceivable. T is really a function of A, and would cease to exist with it.

Direct evidence of this negative curvature is easily obtained by allowing a drop to hang from the end of an open vertical tube of narrow bore, into which a supply of liquid is led by a fine capillary thread; as the drop grows the head of liquid supported may be observed to diminish. Owing, however, to the smallness of the change, the slowness with which the meniscus in a fine tube adjusts itself, and the liability to alteration of the surface tension of the drop through long exposure, the method cannot be used with advantage for measuring the curvature.

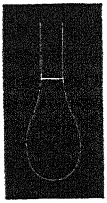
It is important, however, to notice that there is not any physical breach of continuity, or critical point reached, when the pressure at the base becomes zero or negative, which can influence the separation of the drop. For there is no reason to suppose that the small negative pressure finally attained at the base of the drop, varying as it does with the diameter of the tube, represents any physical limit of the cohesion, which is probably very great.

Hence, it is clear that the separation of the drop is to be attributed to the surface becoming unstable for small oscillations, rather than to the cohesion being in any true sense overcome by the force of gravity, and that the cleavage of the drop is analogous to the cleavage of the unduloid or cylinder first studied experimentally by M. Plateau.

It is necessary to insist on this point, for it has been apparently misconceived by Professor Quincke, who has endeavoured to deduce the value of the tension by equating the weight of the drop which falls to the tension multiplied by the circumference of the tube or cylinder from which it hangs, the latter being taken so small that the drop hangs with the uppermost element of the liquid vertical. (See fig. 3.)

The nature of his proceeding will be best seen from the following quotation.*

FIG. 3.



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* From a paper communicated to the Royal Academy of Sciences at Berlin, May 28, 1868, and translated in "Phil. Mag.," 1869, by Professor Jack. The italics are my own.

"The equation (7) $\frac{W}{2\pi r}=T$,

is true also for drops formed at the mouth of a vertical pipe *on the assumption that in consequence of the gradual accession of new fluid the same pressure is found in the interior fluid at the mouth of the pipe as in a level fluid surface.*" "The drop goes on increasing till $W=0$, or till the highest element of the fluid is vertical, and then it falls off.

"If the radius of the cylinder on which the drop is formed be very small, the weight of the portion of the fluid which remains hanging may be neglected, and the weight of the portion of the drop which falls may be treated as the W in equation (7)."

He is here speaking of a drop pendent from a tube so small that it wets the outside and hangs as in fig. 3. It is, however, obvious that, under such circumstances, the surface of the liquid, where vertical, shares the curvature of the tube, and that the equation of equilibrium should be $2\pi r T=W+$the pressure on the area of the section of radius r,

or
$$2\pi r T=W+\pi r^2\frac{T}{r},$$

or
$$\frac{W}{\pi r}=T.$$

In other words, Professor Quincke's value of the tension is precisely half what it ought to be. Hence, the very low value that he obtains for the surface tension of water by this method, ·0548 as compared with ·08253 by other methods.

He applies the same equation to Professor Guthrie's experimental data, although indeed it is not applicable to the larger terminals used by the latter, so that the approximate agreement of the results with those obtained by other methods is really due to a fortuitous cancelling of errors.*

I will now explain the manner in which the tracing may be used for evaluating the pressure at the base, and determining the value of the tension.

It is convenient to assume first that this pressure is zero, then to introduce in the value of T the correction which is found from an examination of the curve to be necessary.

Assuming then that the pressure at the base is zero, it is evident that if any horizontal section of the drop be made, the vertically resolved part of the surface tension across the circumference of this section is equal to the weight of the portion of the drop below the section, plus the pressure of the head of liquid reaching from the section to the bottom of the tube multiplied by the area of the section on which it acts.

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* "Pogg. Ann.," vol. cxxxi, p. 137.

Thus if—

r be the radius of the section,

θ the inclination of the tangent to the axis of revolution,

H the head of liquid in centims.,

V the volume of the part below the section in cub. centims.,

T the surface tension in grams per linear centim.,

D the weight in grams of 1 cub. centim. of the liquid,

then
$$T \cos\theta \times 2\pi r = (V + \pi r^2 H) D,$$
$$T = \frac{(V + \pi r^2 H) D}{2\pi r \cos\theta}.$$

r and H were measured carefully with compasses and a millimetre scale, which could be read by estimation to tenths of millimetres, and $\cos\theta$ was determined directly by the same means after drawing the tangent to the curve. V was obtained by dividing the area below the section into narrow horizontal strips, whose dimensions were measured with scale and compasses, and the volume of the solid generated by the revolution of each, considered as the frustum of a cone, was calculated. It was found in practice that the bottom part of the curve is generally so nearly circular that the best way is to strike the circle of which it is the arc, for other parts of the curve lines, ruled at a distance of 5 or 10 millims. apart (when the magnification was about fifteen times linear), gave a sufficiently accurate approximation, it being impossible to distinguish from straight lines the small portions of the curve intercepted between each pair.

It was easy, by marking the depth, to take two or more independent tracings at exactly the same stage of extrusion, and superposition of these showed very complete agreement, and gave confidence in the result; moreover, the symmetry of the tracing on the two sides of the axis affords a check on the accuracy of the drawing. The tangent is most easily drawn at a point of re-curvature, or where the curvature in the plane of the paper is small; on the other hand, an error in the value of $\cos\theta$ increases in importance as θ increases.

These considerations should influence the choice of sections.

Treating in this way the curve of a drop of olive oil hanging in air, the following values were obtained for the surface tension at the different sections (1), (2), (3), and (4), which are arranged in order of increasing area.

$$T_1 = \cdot 049401,$$
$$T_2 = \cdot 051215,$$
$$T_3 = \cdot 057775,$$
$$T_4 = \cdot 065063.$$

The discrepancy at once shows that the assumption of no pressure due to curvature at the base is wrong, and that the pressure is in reality negative, and that each value of the tension requires to be diminished by a correction which, being proportional in each case to the area of the section, will affect the higher values most, the smaller least. If we call the unknown pressure due to curvature at the base c, and introduce this in the equation, it becomes

$$\mathrm{T}\cos\theta\times 2\pi r=(\mathrm{V}+\pi r^2\mathrm{H})\mathrm{D}-\pi r^2 c,$$

$$\text{or } \mathrm{T}=\frac{(\mathrm{V}+\pi r^2\mathrm{H})\mathrm{D}}{2\pi r\cos\theta}-\frac{rc}{2\cos\theta},$$

so that we have now four equations for determining T and c, viz.,

$$\mathrm{T}=\mathrm{T}_1+\frac{r_1 c}{2\cos\theta_1},$$

$$\mathrm{T}=\mathrm{T}_2+\frac{r_2 c}{2\cos\theta_2},$$

$$\mathrm{T}=\mathrm{T}_3+\frac{r_3 c}{2\cos\theta_3},$$

$$\mathrm{T}=\mathrm{T}_4+\frac{r_4 c}{2\cos\theta_4},$$

any two of which are sufficient.

Combining these equations by the method of least squares so as to obtain the most probable values of the unknown quantities, we get

$c=-$·09943 grm. per square centim.,

$\mathrm{T}=$ ·03373 grm. per linear centim.,

and using these values to calculate the most probable values of the observed quantities T_1, T_2, &c., we have

Calculated.		Observed.
$\mathrm{T}_1=$ ·04935	....	·04940
$\mathrm{T}_2=$ ·05121	....	·05181
$\mathrm{T}_3=$ ·05777	....	·05688
$\mathrm{T}_4=$ ·06506	....	·06538

It remains to observe that, as will appear in the table of results, the pressure due to curvature at the base was found to be negative even where the drop hung from a plane surface of indefinite extent which it wetted entirely.

It must be admitted that the surface here (see fig. 1, Plate 7) really passes into a continuous plane where the curvature, and consequently the pressure due to curvature, is zero; and that, nevertheless, at an indefinitely small distance below this, the pressure due to curvature is undoubtedly negative.

The explanation is, I think, to be found in the fact that the liquid film which wets the surface, is, by reason of the contact, in a modified condition, viscous and unable to transmit pressure like the rest of the liquid.* The existence of such modified covering films seems to have been well established by Professor Quincke.†

I have endeavoured to lessen the labour of calculating the volume of liquid supported by the tension at any section, by cutting out of a uniform sheet of card the area by whose revolution round the axis this volume is generated. The magnitude of this area was found by weighing, and the position of its centre of gravity by balancing on a knife-edge, from which, by applying the well known "Property of Guldinus," the volume of the solid was at once obtained. Owing, however, chiefly to the unequal thickness of even the most uniform material, whether card or metal foil, that I could procure, the results obtained in this way were liable to differ from those obtained by the previously described method of integration by 1 or 2 per cent., so that the method cannot be recommended. I have, however, retained in the following table the measure of T obtained in this way for water in a particular case, and have also inserted for comparison the values thus obtained at different sections of a chloroform drop. The table contains the record of results obtained in the way described.

In most cases a tube was used, the diameter of which is recorded, instead of a plane surface of indefinite extent, since it was found difficult to prevent the drop from gliding about and getting out of focus, when pendent from the latter.

The temperature (when recorded) is that indicated by a thermometer placed close to the drop.

The surface tension (T) is expressed in grams per centim.

Olive Oil (density ·92129).

Indefinite plane. Temperature about 18° C.

Calculated.		Observed.
$T_1 =$ ·04935	....	·04940
$T_2 =$ ·05121	....	·05181
$T_3 =$ ·05777	....	·05688
$T_4 =$ ·06506	....	·06538

$c = -{\cdot}09943$

$T = {\cdot}03373$

---

* It sometimes seemed to me that the rest of the liquid started out of this film at a small but definite angle, of which I obtained the following measures in the case of turpentine hanging from a flat iron terminal, which it wetted to an indefinite distance:—18°, 20°, 21°·5, 20°·5, 19°·2 mean 19°·84.

† "Phil. Mag.," June, 1878.

Alcohol (density ·8001).

Diameter of tube ·9525 centim. Temperature about 18° C.

	Calculated.		Observed.
$T_1=$	·03396	....	·03415
$T_2=$	·03617	....	·03583
$T_3=$	·03821	....	·03834

$c=-·05596$

$T=\ ·02586$

Russian Turpentine (density ·8679).

Diameter of tube 1·153 centim. Temperature about 18° C.

	Calculated.		Observed.
$T_1=$	·040185	....	·040354
$T_2=$	·042123	....	·042088
$T_3=$	·045613	....	·045749

$c=-·081236$

$T=\ ·028179$

Chloroform (density 1·51294).

Diameter of tube ·9525. Temperature about 15°.

Drop No. 1.

	Calculated.		Observed.
$T_1=$	·037113	....	·037354
$T_2=$	·039542	....	·039239
$T_3=$	·048582	....	·048644

$c=-·04438$

$T=\ ·03025$

Drop No. 2.

	Observed.		By weighing.
$T_1=$	·02983	....	·02921
$T_2=$	·03034	....	·02949
$T_3=$	·03074	....	·03005
$T_4=$		....	·02958
$T_5=$	·03046	....	·02965

$T=\text{mean}=\ ·03034$

Here the difference between the values at different sections was so small that c was neglected and the mean taken.

Water.

Diameter of tube ·9525 centim. Temperature —

Calculated.		Observed.
T_1= ·098829	....	·098804
T_2= ·092912	....	·09324
T_3= ·098829	....	·098804

$c = -$·07625

T= ·080056

Diameter of tube ·9525 centim. Temperature about 18° C.

T_1= ·078802
T_2= ·077588
T_3= ·078349
T_4= ·078592

Here the difference was so small that the correction was neglected and the mean taken—

T= ·078327.

In saturated air. Temperature about 15° C. December 4, 1880.
Diameter of tube 1·252 centim.

Calculated.		Observed.
T_1= ·09376	....	·09377
T_2= ·09677	....	·09648
T_3= ·010005	....	·010001

$c = -$·12547

T= ·07359

Water (continued).

Indefinite plane. October 15, 1880.

Calculated.		Observed.
T_1= ·10158	....	·10142
T_2= ·10999	....	·11023
T_3= ·11528	....	·11522

$c = -$ ·11600

T= ·07383

By weighing.

Diameter of tube ·9525. Temperature 4° C.

	Calculated.		Observed.
$T_1=$	·07598	....	·07516
$T_2=$	·07685	....	·07797
$T_3=$	·07930	....	·07897

$c = -$ ·03030

$T=$ ·07180

It will be observed that the values of T for water vary very considerably. Moreover, on no occasion have I obtained so high a value as the mean ·08253 deduced by Professor Quincke by the method of flat bubbles.

The agreement of the measures is in each case so close as to leave no doubt that the surface tension of the liquid really differed considerably on different occasions. I have at present no better explanation of this to offer than the fact that the surface tension of water is particularly liable to diminution by exposure to the impurities of the atmosphere.

To aid a comparison of the method with that of flat drops or bubbles, on which Professor Quincke seems chiefly to rely, I append a table giving the values of the tensions by the two methods in parallel columns. It is to be observed that the liquids used are often not quite the same, as is indicated by the difference of density.

The name of the observer is signified by the initial letter.

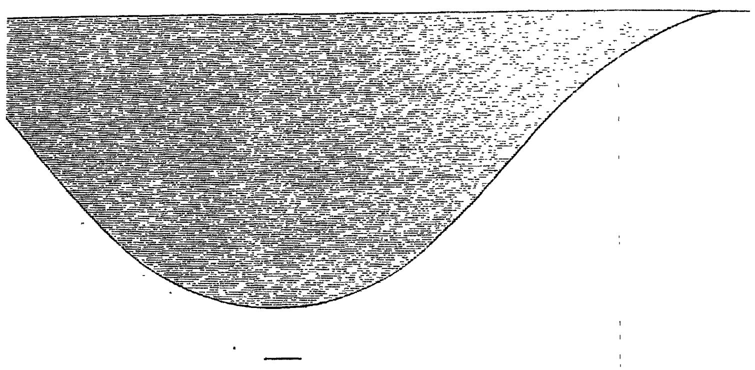
Name of substance.	Density.		Surface tension in grams per centim.	
	Q.	W.	Q.	W.
Water	1	1	·08253	·0718 to ·08005
Alcohol	·7906	·8001	·02599	·02586
Turpentine	·8867	·8679	·03030	·028179
Olive oil	·9136	·92129	·03760	·03373
Chloroform	1·4878	1·5129	·03120	·03025

Professor Quincke's result in the case of water is the mean of seven measures ranging between ·0800 and ·0892; that for turpentine is the mean of eight ranging from ·0284 to ·0318; that for olive oil is the mean of seven, ranging from ·03617 to ·04113.

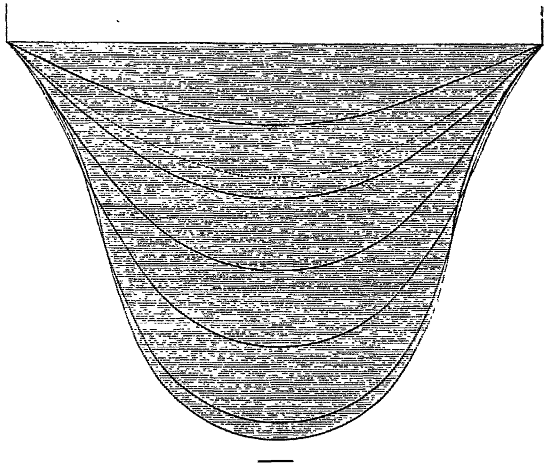
The method of flat drops and bubbles is, as he himself points out, only approximate, since the curvature due to revolution is neglected, and is liable to give results that are rather too high.

WATER.

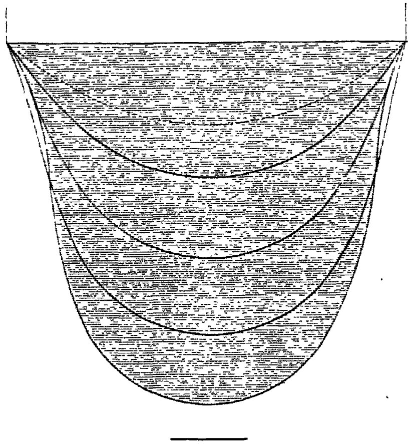
Indefinite plane (glass)



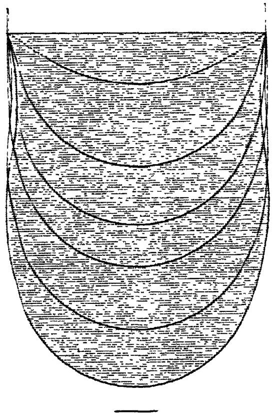
Diam. $= \overset{\text{cm.}}{\cdot 9525}$.



Diam. $= \overset{\text{cm.}}{\cdot 724}$

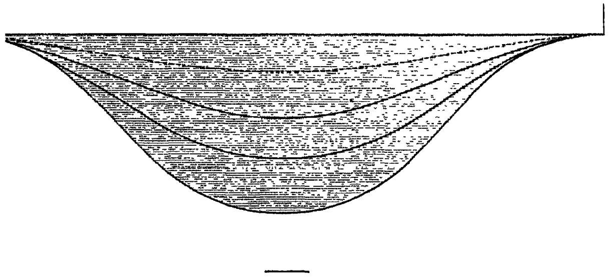


Diam. $= \overset{\text{cm.}}{\cdot 448}$

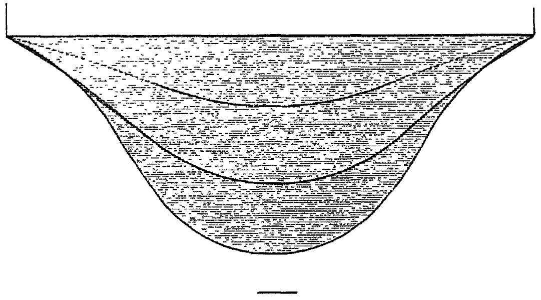


TURPENTINE (*Density. ·8679*)

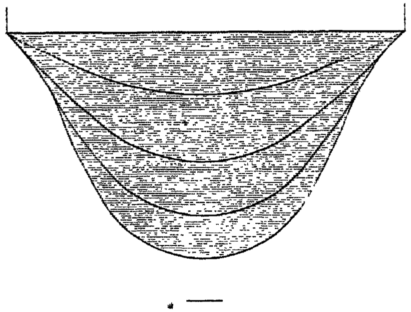
Diam. $= \overset{\text{cm.}}{1.153}$.



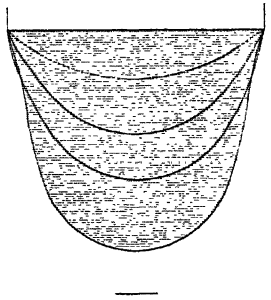
Diam. $= \overset{\text{cm.}}{\cdot 9525}$.



Diam. $= \overset{\text{cm.}}{\cdot 7238}$.



Diam. $= \overset{\text{cm.}}{\cdot 448}$.



West, Newman & C° lith.

The method which I am advocating is open to no such objection; it is moreover rapid, allowing little time for the alteration by exposure of the surface under consideration, and it is independent of the very difficult determination of the contact angle of liquid and solid, which enters into some other methods.

At the same time the results are probably not so accurate as could be obtained by the use of photography for recording the form of the drop.

I am in hopes of obtaining, by its means, results which may, in some measure, serve to verify the fundamental assumptions of the theory on which the value of the so-called surface tension is calculated.

Of the phenomena, to which I have ventured incidentally to call attention, that of the approximate constancy of the drop-depth, requires closer investigation than I have been able to give it, as it seems to offer a very satisfactory means of obtaining a first approximation to the value of the surface tension. In all experiments on this subject the scrupulous cleanliness so absolutely necessary renders progress slower than many would suppose, which is my excuse for presenting an investigation, which I trust may be found useful, though obviously incomplete.

III. "Postscript to the Chronological Summary of Methods of Computing Logarithms in my Paper on the Potential Radix." By ALEXANDER J. ELLIS, B.A., F.R.S., F.S.A. Received May 18, 1881.

After the publication of my "Chronological Summary of Methods of Computing Logarithms," in the "Proceedings" for 3rd February, 1881 (vol. 31, pp. 401-407), Mr. Isaac Todhunter, F.R.S., kindly pointed out to me an almost unknown pamphlet by George Atwood, F.R.S., inventor of "Atwood's Machine," which he informs me is not mentioned even by Dr. Thomas Young, in his account of Atwood. A copy of the pamphlet is in the library of the Royal Astronomical Society, and at the request of Mr. Peter Gray, F.R.A.S., was kindly lent to me for inspection. The following account of this important and little known work should be inserted in my summary, on p. 403, between 1771, *Flower*, and 1802, *Leonelli*:—

1786. *Atwood*, George, F.R.S. "An Essay on the Arithmetic of Factors, applied to various computations which occur in the practice of numbers." This is essentially a method, when any number A is given, of finding factors, M, p, q, r, s, t, such that $A \times M$ being very nearly $=1$, of the form $\cdot 9\ldots$ or $1\cdot 0\ldots$, and p, q, r, s, t being of the form $1 \pm \cdot 0_n k$ (where 0_n means n successive zeroes, as in the notation of my paper, or, as Atwood writes $1\,.\,[n]k$ or $\overset{+}{1}\,.\,[n]k$) then $1=AMpqrst$, to any required number of decimal places. Atwood works to 13 places to secure 12, and applies the result to extraction of roots, &c., as well as to the computation of logarithms and anti-logarithms, which can alone be noticed here. It is evident that when these factors are once found $-\log A=\log M+\log p+\log q+\ldots$ to any base, and that p, q, &c., and $\log p$, $\log q$, &c., will be numbers and logarithms in a positive or negative numerical radix (vol. 31, p. 398) according as p, q, &c., are of the form $1+{\cdot}0_nk$ or $1-{\cdot}0_nk$.

If B=the first or two first figures of $10^n\div A$, then we may take $M=B\div 10^n$, where n is the number of places in the integer of A, or $-n$ is the number of zeroes between the decimal point and the first significant digit. This determination of M is an anticipation of the "réciproques approchés" of Thoman, 1867, and Atwood's Table IV of the values of B is equivalent to the two first columns of Thoman's Table I, and of Hoppe's Table IV. The number is thus reduced to the form $\cdot 9\ldots$ or $1{\cdot}0\ldots$

Taking the first case, where the reduced number is less than 1, Atwood multiplies it "up to 1," which is a revival of Flower's reflected rule, with which Atwood was clearly not acquainted. But instead of retaining the reduced number in the form $\cdot 9\ldots$, and finding the factors $1{\cdot}0_nm$ as Flower did, by taking $m=$ the excess of 9 over the corresponding digit in the number, Atwood transforms it into the form $1-{\cdot}0\ldots$, by subtracting each digit, except the last, from 9, and the last from 10. This is an anticipation of Hoppe's artifice, 1876. In this case the values of m in $1{\cdot}0_nm$ are the successive digits of $1-{\cdot}0\ldots$ themselves.

The product of the resulting factors, say, $1{\cdot}04$, $1{\cdot}0_36$, $1{\cdot}0_41$ (or in Atwood's notation $1\,.\,[1]4$, $1\,.\,[3]6$, $1\,.\,[4]1$), were not written as a product but as $1{\cdot}\underline{04061}$, the line underneath indicating that it is not a decimal fraction, and the position of each digit indicating the number of preceding zeroes in the corresponding factor. The figures underlined correspond precisely to Flower's "line of ratios." To find the nat. log of this product $1{\cdot}\underline{04061}$, Atwood observes that it is very nearly $\cdot 04061$, which, however, is too large, and requires correction, being in fact the sum of the first terms of the nat. logs of the factors. This correction he finds by assuming that $\log 1{\cdot}0_61={\cdot}0_61$ to 13 places, and similarly for all smaller numbers, which determines the kind of logarithm, and then he finds the logarithms of larger numbers in his radix by resolution into factors. Finally he tabulates only the differences or ${\cdot}0_nm-\text{nat.}\log(1+{\cdot}0_nm)$ in his Table I, which is thus identical with Hoppe's Table III to the first 13 places. By this table and resolution into factors, Atwood calculates log B (where B consists of two digits, and is between 1 and 10), and his Table III, the equivalent of Hoppe's Table II, and (with the substitution of natural for tabular logarithms) of Thoman's Table I, col. last. Atwood's first case is therefore precisely the same as Hoppe's method, having identical tables but a different arrangement of computations and notation.

Taking the second case, where the reduced number is greater than 1, Atwood multiplies it "down to 1." The process of finding the factors is precisely that of Weddle, 1845; Hearn, 1847; Thoman, 1867; and Wace, 1873; exemplified in my former paper (vol. 31, p. 400). The product of the factors $(1-\cdot04)$, $(1-\cdot0_36)$, $(1-\cdot0_41)$, is written $\overset{+}{1}\cdot\underline{04061}$, for which might be substituted $1-\cdot\underline{04061}$, where $-\cdot04061$ is the sum of the first terms of the natural logarithms of the factors, which in this case are all negative. Hence on calculating a table of $-\cdot0_nm$ —nat. log $(1-\cdot0_nm)$, which is easily done by the first case, we find the corrections. These are given in Atwood's Table II, which has not been given by any subsequent writer, because Hearn and Thoman, using tabular logarithms, gave the complete value of the tab. log of the factors, instead of corrections for the first term.

Hence it appears that Atwood, 1786, rediscovered Flower's method, 1771, but transformed it in the manner carried out ninety years later by Hoppe, 1876; and not only anticipated Weddle's method, 1845, but showed the connexion of the two methods, as that of multiplying the reduced number "up to 1" in the first case, and "down to 1" in the second. To Atwood, then, must be attributed the real discovery of these methods, their correlation, and their subordination to a general principle of an "arithmetic of factors," and the merit of having calculated his tables, as he says (p. 36), "without reference to the hyperbola, binomial theorem, or any fluxional process, by multiplication only, the elementary properties of ratios and of powers and their indices being granted."

ERRATA.—The following slight errors escaped correction in my former papers: vol. 31, p. 397, l. 1, *for* The preparation *read* In Ex. 2 the preparation; same page, l. 18, *for* Here *read* In Ex. 3; p. 398, l. 15 from bottom, *for* Wolframm *read* Wolfram; p. 399, l. 6, *read* paper, by the positive numerical radix, and dele l. 9; p. 405, l. 9 from bottom, *read* (suprà, p. 382); p. 406, l. 6, *read* **Gray*; *ibid.*, l. 19 from bottom, *read* †*Pineto*; *ibid.*, l. 17 from bottom, *read* Petersburg; and l. 9 from bottom, *read* Thesaurus, and p. 409, l. last, *read* gives (on p. 412).

IV. "Note on the Spectrum of Carbonic Acid." By CHARLES WESENDONCK (Berlin). Communicated by Professor HELMHOLTZ, For. Mem. R.S. Received May 16, 1881.

The well-known spectrum first observed by Swan at the inner cone of a number of flames produced by burning liquid and gaseous combinations of carbon, has been ascribed by Messrs. Attfield[*] and Watts[†] to the element carbon itself; while several other natural philosophers, amongst whom I only mention Messrs. Thalén and Ångström,[‡] P. Smyth, Liveing[§] and Dewar,[||] believe it to be due to some combination of carbon with hydrogen. The fact that the bands and lines of Swan's spectrum are seen even when oxide or binoxide of carbon is made luminous in a Geissler tube by means of an electric current, the last-named gentlemen explain as being caused by the gases not having been entirely deprived of all moisture when introduced into the vacuum tube. I therefore believed it to be not altogether without interest to make some experiments on carbonic acid dried with the utmost care. For this purpose I used a mercurial pump, as invented by Professor Toepler, with some slight changes in the original construction indicated by Dr. Hagen,[¶] which enables us not only to obtain the most perfect vacua, but also to avoid every cock or junction by slided pieces, which must be greased in order to be hermetic, so that no vapours and gases containing carbonic matters arising from the grease, either by increase of temperature or by the electric current when passing the vacuum tube, would render impure the carbonic acid, as is the case with the common Geissler pumps. Carbonic acid was produced in the well-known manner from marble and diluted muriatic acid in a Kippian apparatus, well washed with water in a large Woolf's bottle, and then sent through concentrated sulphuric acid, three tubes of U form containing chloride of calcium, and a double tube half filled with anhydride of phosphoric acid. To this last tube were fastened, by means of sealing, two Geissler glass cocks, being on the other side fastened by sealing to a valve of sulphuric acid, that prevented all vapours coming from the cocks from entering the vacuum tube. After all air had been driven and pumped out of the whole apparatus, the spectrum tube was evacuated to the highest possible degree, in order to free it from all moisture adherent to the sides of the glass. This, however, was found to cause some difficulty, for the green hydrogen line was seen with great constancy in high vacua, even if not the least trace of it could be remarked when the carbonic acid spectrum could be seen brightly. I soon recognised that the more the gas had been deprived of moisture, the further had the exhaustion to be carried on to render the said line visible again, but for a long time I could not get quite rid of it. By repeatedly filling and exhausting the vacuum tube, in doing which the gas naturally was only permitted to pass very slowly through the phosphoric anhydride tube, and the spectrum tube was heated to the highest degree possible, I at last arrived at such a degree of perfect dryness as no more to give any trace of the fatal green line, not even if the tube was exposed to high temperature; this, indeed, always had the effect of driving into the vacuum some matter adherent to the sides of the glass, and causing the illumination of the tube, when this had been so far exhausted as no longer to permit any current to pass, to reappear. But the brilliant mercury spectrum seen under such circumstances showed nothing of a hydrogen line. Nevertheless, after this exceedingly high degree of dryness had been obtained, when there was some gas admitted into the spectrum tube and this again exhausted, the fatal line was feebly seen, showing clearly that the gas had not been entirely deprived of its moisture by its passing through the system of drying apparatuses. I therefore finally only experimented with such carbonic acid as had been in perpetual contact with the phosphoric anhydride during at least twelve hours, and it was not till now that I got rid of every trace of the hydrogen lines. When making the experiments that I believe to be decisive, I always took care to convince myself, by rarefying, not only that the gas had remained perfectly dry, but also that the current had not introduced any trace of hydrogen or hydrocarbon when passing through the tube; for both would have been discovered in the high vacuum by the appearance of the green line, the spectrum of hydrogen being by no means oppressed by that of carbon in such a vacuum, as I most clearly observed when experimenting with olefine gas, which in a very rarefied state shows from the beginning of the current's passage a very distinct spectrum of the first order of hydrogen, besides the one of carbon. If the bands and lines observed by Swan really belonged to hydrocarbon only they could not have appeared in our case, but they did appear very distinctly and clearly, as soon as the density of the gas became great enough to oblige the current to pass as a spark through the vacuum tube; and even not less brightly was Swan's spectrum seen at much lower pressures by using a Leyden jar. The capillary tube in which the said spectrum alone was seen was then wholly filled by the luminous discharge; no real spark appeared in this case, while the wide parts of the spectrum tube showed Watts' second carbon spectrum. It is these last-mentioned experiments only, made by use of a Leyden jar, that I believe to be decisive, and to prove irrefutably that Swan's spectrum cannot be ascribed solely to hydrocarbon. As to the phenomenon shown by sparks, it is not quite so convincing; for, accepting Wüllner's* ideas, one could object that the spark affecting only a set of molecules of infinitely small transverse section could have carried forth from the electrodes an exceedingly small quantity of hydrogen, which would quite escape observation when expanded throughout the whole vacuum tube, but would be sufficient to induce the hydrocarbon spectrum in the line of the spark. This explanation, however, I do not think to be at all probable. The tube used in these experiments was of common size; the ends of the electrodes, which were of aluminium, had a distance of about 15 centims. from one another. Some results, differing from those just mentioned, were obtained in a wide spectrum tube without capillary tube, the ends of its electrodes being distant about 3 centims. from one another. The spark then no longer exhibited Swan's spectrum, even when the carbonic acid was not dried with the utmost care, as in the above-mentioned researches; but there were seen the enlarged maxima of Watts' second spectrum, which I have shown by some researches, of which a short account has been published in the "Berlin. Monatsb.," 1880, p. 791, to be generally due to the continuous form of discharge, according to the nomenclature of Messrs. Thalén and Ångström. If the density of the gas was still increased, there flashed a very brilliant line spectrum, never before observed by me, in a tube of common size. These facts, at all events, show that there exists some relation between the different orders and forms of spectra and the conditions of discharges (probably, according to my opinion, the quantities of electricity sent through the unit of space in the unit of time, till now unknown to us) that will doubtless become clear as soon as the causes governing spark discharges shall be better recognised—a subject on which Sir W. Thomson† says that it is difficult even to conjecture an explanation.

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[*] "Phil. Trans.," vol. 152, 1862.
[†] "Phil. Mag.," Ser. 4, vol. 38, 1869.
[‡] "Nova Acta. Upsal.," Ser. 3, vol. 9.
[§] "Phil. Mag.," (5), 8, 1879.
[||] "Proc. Roy. Soc.," vol. 30, 1880.
[¶] "Wiedemann's Annalen," Neue Folge, Bd. 12, 1881.

V. "The Wave of Translation and the Work it does as the Carrier Wave of Sound." By JOHN SCOTT RUSSELL, F.R.S. Received June 13, 1881.

Synopsis.

Short history of the wave of translation discovered by me in 1832–3. Investigation of its shape, nature, speed, and difference from all other waves in its character as a solitary carrier wave.

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* Wüllner, "Lehrbuch der Experimental Physik," vol. 2, p. 250, &c.

† Papers on "Electrostatics," &c., p. 247.

The application of this solitary wave in air to the carrying of sound.

Errors of the phrases in general use for the explanation of the conveyance of sound.

My theory of the mode in which sound is propagated as distinguished from the mode of its creation.

Consideration of the aerial wave of sound, its analogy to the solitary wave in water.

The *depth* of the air ocean and its correspondence to that of the water ocean.

The speed of the solitary or carrier wave in each element. The speed is due to the depth. What happens in the interior of the solitary wave in water? The same motions take place in the same nature of aerial wave.

Elucidation by the comparison of three equivalent oceans.

Similarity and dissimilarity between the oscillating waves and the carrier or solitary wave.

Musical sounds in their melodic and harmonic relations to this wave. Numerical nomenclature of sounds.

How the message of the sound wave is delivered to the brain.

VI. "On the Effect of Electrical Stimulation of the Frog's Heart, and its Modification by Cold, Heat, and the Action of Drugs." By T. LAUDER BRUNTON, M.D., F.R.S., and THEODORE CASH, M.D. Received May 16, 1881.

(Abstract.)

From the results of the recorded experiments conducted on the frog's heart in its normal position and still exercising its circulatory function, we have found—

I. That electrical stimulation by a single induced shock has either an obvious effect on the contraction and rhythm of the organ, or no such effect is apparent.

II. That the effect is modified by—

(a.) *The time of the cardiac cycle in which stimulation falls.*—Thus Marey has already shown that a so-called refractory period is demonstrable under certain conditions.* Well-marked variations in latency when the stimulation is potent to induce a systolic contraction are to be recognised.

(b.) *The strength of the stimulation applied.*—Refractory periods possible under minimal stimulation can no longer be demonstrated under maximal, whilst a disturbance of the relationship of auricular and ventricular contractions may be induced.

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* The conditions of this refractory period, or "period of diminished excitability," have been very fully investigated by Dr. Burdon Sanderson and Mr. Page. "Proc. Roy. Soc.," vol. 30, p. 373.

(c.) *The area of the heart to which stimulation is applied.*—A refractory period demonstrable under stimulation of the ventricle may cease to occur when the sinus venosus is the seat of irritation.

(d.) *The action of heat, cold, and drugs.*—Thus cold prolongs the systole, the refractory period, and the latency of an induced contraction; whilst strychnia, leaving the curve of systole unaltered, lengthens the refractory period to a marked degree.

VII. "On the Action of Ammonia and its Salts, and of Hydrocyanic Acid upon Muscle and Nerve." By T. LAUDER BRUNTON, M.D., F.R.S., and THEODORE CASH, M.D. Received May 26, 1881.

(Abstract.)

Ammonia.—The curve caused by stimulating either muscle or nerve is prolonged. Fatigue increases this prolongation much more rapidly than it would do in the case of a healthy muscle. The viscosity, or residual contraction, remaining after active contraction is much more increased in the case of direct (muscle) than of indirect (nerve) stimulation.

Chloride of Ammonium.—A powerful, though slightly lengthened, contraction occurs on direct stimulation. The muscle, as stimulated from the nerve, yields a few (if any) lengthened and feeble contractions, and then ceases to respond altogether to stimulation.

Nitrate of Ammonium.—The nerve, in this case, is not more affected by the poison than the muscle. If the poisoning be very complete, neither direct nor indirect stimulation has any effect; if it is not so, the curve is observed to be prolonged. In the case of both, but especially in that of the directly stimulated muscle, the contraction remaining after stimulation is very extensive.

Nitrite of Ammonium.—If poisoning be complete, no reaction occurs either on direct or indirect stimulation; if not, the curves of both are much prolonged, even more so than in the case of the nitrate. The rapid repetition of stimulation causes the curve to become enormously prolonged, whilst the viscosity after the active contraction is much increased.

Cyanide of Ammonium. — Stimulation of the nerve produces no effect whatever; whilst that of the muscle causes a considerable though prolonged curve. Viscosity is much increased. The curve of hydrocyanic acid alone, yielded by both direct and indirect stimulation, is prolonged, but it neither shows the length nor the viscosity of the cyanide muscle curve; whilst its nervous contraction is still powerful in contrast to the utter loss of excitability in the latter.

The *latency* varies in the case of the different salts of ammonia. Thus, in stimulating the nerve, it is much lengthened in the chloride, and still more so in the cyanide—if the muscle be examined soon enough to yield response to nerve stimulation—it is increased slightly by ammonia, and by its nitrate and nitrite. In direct stimulation, the latency appears to be increased in the cyanide nitrate and nitrite, unaltered in the chloride and hydrocyanic acid, and unaltered or slightly decreased in the early stages of poisoning by ammonia.

VIII. "On Stratified Discharges. VI. Shadows of Striæ." By WILLIAM SPOTTISWOODE, President R.S., and J. FLETCHER MOULTON, F.R.S. Received May 27, 1881.

One of the most interesting questions connected with the subject of stratified discharges is this: What is the physical, as distinguished from the electrical, nature of the striæ themselves? Are they, in fact, to be regarded as aggregations of matter possessing greater density than the gas present in the dark spaces, or are they to be considered as indicating merely special local electrical conditions? The fact of their having a definite configuration, especially on the side which is turned towards the negative terminal of the tube, that of their temperature being higher than that of the dark spaces, the manner in which they are affected by solid bodies, and other considerations, all tend to give support to the view that the striæ are loci of greater density than the dark spaces. Still it can hardly be said that as yet any experimental proof of this has been given, sufficiently decisive to decide the question conclusively. And it is in the hope of contributing something towards the solution of this question that the following experiments are submitted to the notice of the Royal Society.

In the former papers of this series ("Proc. Roy. Soc.," vols. 23, 27), and in our memoirs "On the Sensitive State of Vacuum Discharges" ("Phil. Trans.," 1879, p. 155, and 1880, p. 561), we have described experiments which show how a discharge through a tube, whether sensitive or non-sensitive, may be affected in various ways by intermittent discharges *ab extra*. In the experiments now to be described the problem has been reversed, and it has been proposed to ascertain how far such an external discharge can itself be affected by the internal one. The discharge *ab extra*, however, presents such feeble luminosity, that it is not possible to come to any certain conclusions on this head by direct methods. But, as has been before pointed out, the intermittent discharge *ab extra* is eminently calculated to produce Crookes' green phosphorescence, and the effect produced on this phosphorescence by the striæ of the internal discharge proves to be not only well marked, but even striking, and of this we now propose to give an account.

In the second of the two memoirs above quoted, Section 14, we studied "the effect of intermittent inductive action of an impulsive type upon continuous discharges." In the present instance the instrumental disposition was not very different from that which was there described. The two terminals of a Holtz machine were connected in the usual way with the two terminals of the tube, so as to produce a stratified discharge. A narrow strip of tin-foil, or a wire, was stretched along the tube opposite the column of striæ, but not extending much beyond it. The reason for the latter precaution will be mentioned below. The positive terminal of a second Holtz machine (in practice we used for this purpose a Töppler machine) was connected with the tin-foil, and the negative terminal with one (either) terminal of the tube. An air-spark, or interval across which sparks could pass, was interposed in the part of the circuit between the machine and the tin-foil. The effect of this arrangement was similar to that described in our first memoir, p. 170, viz., in the interval between two sparks the tin-foil and tube became charged like a Leyden jar; the tin-foil being the outer coating, charged positively, and the gas inside serving as the inner coating, charged negatively. When the spark passed across the interval mentioned above, the jar (*i.e.*, the tube) became discharged, and the electricity previously held bound on the two coatings was set free.

When the first (say the "internal") machine was not working, or when it was disconnected, *i.e.*, when no regular discharge was passing through the tube, then, whenever a spark passed at the second (or "external") machine, a negative discharge with its accompanying Crookes' radiation took place from the inside of the tube next the tin-foil, and the opposite side of the tube became covered with a sheet of green phosphorescence (the tube being of German glass). The length of the sheet was equal to, or very slightly greater than, that of the tin-foil strip, while the breadth was measured by the angular dispersion from the strip, as described in the second memoir.

When, however, other things remaining as before, a discharge from the internal machine was sent through the tube, and a good stratified column was produced, it was found that the green phosphorescence was entirely cut off from the parts of the tube opposite to the striæ, while on the parts opposite to the dark spaces it remained, in the form of phosphorescent rings, at least as brilliant as before. The experiment was repeated with various tubes with various degrees of strength of current, and with various densities of gas (produced by heating a chamber of potash in connexion with the tube). It may be added that when, as is sometimes the case, through greater exhaustion, the striæ became feebler in illumination and less compact in appearance, the shadows cast by them lost proportionally in sharpness of definition and in completeness of extinction of the phosphorescent light.

The reason for not extending the tin-foil beyond the column of striæ is, that when this is done the negative discharge from inside the strip combines with that going on in the main dark space behind the anchored striæ in such a manner as to shorten the column by the length of the strip. When this takes place, as is often, although not always the case, the experiment is frustrated.

The brilliancy and definition of the phosphorescent rings may be increased by inserting a small Leyden jar in the circuit, care being taken that the jar shall discharge itself completely each time. If this is not the case, the main discharge is followed by subsidiary discharges, which tend to blur the effect. The angle of dispersion may be increased, or rather supplemented, by placing more than one strip on the tube, distant from one another by an angle of 90° or 120°. By this means the rings may be made to comprise the entire circumference of the tube.

It should be here mentioned that if the striated column be carefully examined at the moment when the external discharge passes, it will be noticed that the entire column (or more strictly speaking the part of it lying between the positive terminal and the end of the tin-foil nearest to the positive terminal) undergoes a slight displacement. This displacement amounts usually to half the distance between two striæ. The actual shadows cast in the green phosphorescence are those due to the displaced striæ, as may be easily verified.

It thus appears that the striæ are competent to cast shadows in the radiant showers issuing from the inside of the tube adjacent to the tin-foil, which part acts as a negative terminal. Many experiments have contributed to show that these radiant showers, although accompaniments of the discharge, are not carriers of the discharge; and that, having once issued from their source, they continue their own course irrespective of that of the discharge proper. They are in fact material showers, and, although not improbably charged with electricity, yet their ulterior course does not appear to depend on their electrical condition. Under these circumstances the most probable explanation of the phenomena above described appears to be that the showers are arrested by material obstacles in the shape of striæ; and consequently if that be the case, we here have an experimental proof that the striæ represent local aggregations of matter, and not merely special electrical conditions of the gas.

IX. "On Stratified Discharges. VII. Multiple Radiations from the Negative Terminal." By WILLIAM SPOTTISWOODE, P.R.S., and J. FLETCHER MOULTON, F.R.S. Received June 16, 1881.

In our memoirs "On the Sensitive State of Vacuum Discharges" we have often alluded to, and even insisted on, the importance of the remarkable dissymmetry which manifests itself in electrical discharges in gases at low pressures. As the pressure is lowered this dissymmetry becomes more and more marked; the striæ themselves become individually unsymmetrical, and recede one by one into the positive terminal; the features which are associated with the negative increase in importance, until at last they occupy almost the entire area of the tube. The researches of De La Rue, Crookes, Goldstein, and others, have intended to increase the interest which attaches to the action which takes place at the negative terminal. And it is with a view of adding one more contribution to the interpretation of the phenomena of the negative discharge and the analysis of its nature and *modus operandi* that the present experiments are described.

On examining the image of a negative terminal as traced out in tubes of great exhaustion, by the phosphorescence due to Crookes' radiations, we have often noticed that the image was not a simple figure, but that more than one outline of the contour of the terminal might be traced. From the fact of the double contour having been first remarked, where the terminal was of a conical form, it was at first supposed that the second image might be due to internal reflexion, or to some property appertaining to the edge of the cone. But this supposition led to no satisfactory explanation of the phenomenon.

It was, however, thought that, inasmuch as the two images implied different systems of radiation, a magnet, suitably disposed, might affect them in different degrees, and thereby throw some light on their origin. For this purpose we used a large electro-magnet with its coils so coupled up as to give the two poles similar polarity. By bringing the two poles together, inclined at a moderate angle, a single pole and a field of great magnetic strength was produced.

The tube was then placed in the plane containing the axis of the two poles, and in the direction of a line bisecting their directions. The tubes first used were of great exhaustion, and were placed sometimes with the positive, sometimes with the negative terminal towards the magnet. When the tube was placed in a comparatively weak part of the field, the two images of the cone were seen in their usual positions relatively to each other, except that they were slightly more separated. But as the tube was brought gradually into a stronger part of the field, the two images became further separated, and by degrees a third, a fourth, and even more images were brought out on the side of the tube. In one tube of very high exhaustion, for which we are indebted to Mr. Crookes, as many as eight images became visible.

The phenomena here described having been once clearly seen in tubes of high exhaustion, with conical terminals, and under the influence of a very powerful magnet, were afterwards distinguished in other tubes of less exhaustion, with other forms of terminal, and in a less powerful magnetic field.

In the absence of a magnet, the lines of radiation are approximately (though not exactly) normal to the surface of the negative terminal for all the images; and in a magnetic field their tendency is to follow the lines of magnetic force which pass through the point of issue from that surface. The effect of the magnet, therefore, whether the positive or the negative terminal of the tube is presented to the pole, is to direct the lines of radiation more nearly along these lines of force than they would be without its action; and since lines of weaker radiation would be more affected than lines of stronger radiation it follows that different images, formed by lines of different strength (or velocity of discharge) would be separated from one another by the magnet. A similar result would follow if they differed in other respects in their normal condition.

We have then, as an experimental fact, a series of images, each formed by a system of rays issuing from the surface of the negative terminal. The images being distinct, the system of rays must be distinct also. Now, as it seems hardly possible to imagine that, from every point of a surface, there can issue at one and the same instant of time a variety of systems of radiations, each system ranging over a finite angular distance, and each differently directed in space, we are driven to the conclusion that these radiations must have issued successively and not simultaneously from the terminal. In other words, the various images are formed in succession. Now, the entire series of images are present whenever a discharge passes through the tube; and when a "continuous" discharge (such as that from a Holtz machine) is passing, they are all as steady and as persistent as are any other features of the discharge. From this it follows that the radiations are not a continuous phenomenon, but that they are composed of a recurrent series of discharges, each having its own angular range, and its own direction in space; and as the electricity, which is the motive power, and the metallic terminal, which is the directing machinery, are the same in kind for each image, we are led to the conclusion that the positions of the images are determined by the force with which the radiations are projected. In fact, we understand that the various images are due to a succession of discrete discharges of successively diminishing strength.

The phenomenon of multiple images of the negative terminal as explained above has an important bearing on the nature of electrical discharges in vacuum tubes. For, if the phosphorising radiation consists of a recurring series of discrete discharges, the radiation in each series, and *à fortiori* the radiation as a whole, is discontinuous; and consequently the electrical discharge, to which it is due, must itself be discontinuous or "disruptive." We appear, therefore, in these phenomena to have an experimental proof, independent of and in addition to those adduced by Mr. De La Rue and others, of a fundamental point in the theory of these discharges, namely, their disruptive character.

The conclusion, that each main discharge consists of a series of minor discharges, has an interest as throwing light on a difficulty that may perhaps have been felt in respect of one of the conclusions in our second memoir. It was there shown that when the exhaustion was carried to a great degree of perfection, the negative part of the discharge showed signs of a "durational character;" and the difficulty consists in associating the character of duration with that of disruptiveness, as attributed to one and the same discharge. But if the explanation of the multiple images here suggested be correct, it may be possible to reconcile the two ideas by considering that the duration is to be understood not as implying a continuous process, but as measuring the interval from the commencement of each main discharge until the last of the series of minor discharges belonging to it.

This idea of rapidly recurring elemental discharges going to form a single discharge is very similar to the state of things which one of the authors of the present paper showed to take place in the case of a discharge from a coil. (See "Proc. Roy. Soc.," vol. 25, p. 73.) The revolving mirror showed that each discharge from the coil, though apparently single, and, if not instantaneous, at least continuous during the time it lasted, was, in fact, made up of a regular series of elemental discharges, each group of such discharges (constituting a single coil discharge) being composed of like elements. The phenomena above described go to show that even the so-called continuous discharge is made up of very rapidly recurring discharges, each of which is in its turn composed of a group of elemental discharges, the successive members of which bear definite relations to each other.

X. "Note on the Existence in the King Crab (*Limulus polyphemus*) of Stigmata corresponding to the Respiratory Stigmata of the Pulmonate Arachnida, and on the Morphological Agreements between Limulus and Scorpio." By E. RAY LANKESTER, M.A., F.R.S., Jodrell Professor of Zoology in University College, London. Received May 26, 1881.

I.—THE AGREEMENTS BETWEEN LIMULUS AND SCORPIO.

The conclusion arrived at by Straus Dürckheim (1828) to the effect that *Limulus* is an Arachnid, and as such to be placed in a distinct order of the class Arachnida, has not as yet been generally accepted by zoologists. It is, however, to be noted that whilst the zoologists of the first half of this century had no hesitation in placing *Limulus* among the Crustacea, and even in associating it in one order with such Crustaceans as the Phyllopoda, yet there has been an increasing tendency within the last five-and-twenty years to recognise very distinctly something like affinity between *Limulus* and the Arachnida. This tendency has shown itself in the proposal made by some eminent authorities of late (Haeckel, Dohrn, Claus, Gegenbaur) to place *Limulus* in a distinct group by itself (Gigantostraca or Pœcilopoda), neither Crustacean nor Arachnidan, but nearly equally removed from and equally related by supposed genetic connexions to both the Crustacea and the Arachnida. Professor Edouard Van Beneden, of Liège, alone, professedly basing his conclusion upon an examination of the embryological history of *Limulus*, has categorically declared that "the *Limuli* are not Crustacea; they have nothing in common with the Phyllopoda, and their embryonic development presents the greatest analogies with that of the Scorpions and of the other Arachnida, from which it is not possible to separate them."

I have elsewhere* stated my full concurrence with Professor Van Beneden's conclusion on this subject, and proposed accordingly to divide the Arachnida into the Branchiopulmonata and Tracheopulmonata, but I have not hitherto taken an opportunity of setting forth the grounds upon which I am led to hold that the *Limuli* are Arachnida and not Crustacea. These grounds are entirely independent of those advanced by Professor Van Beneden, which were derived from the phenomena of embryonic development. The grounds upon which I base my conclusions are, on the contrary, found in the facts of the adult structure of *Limulus* on the one hand and of the pulmonate Arachnida on the other, more especially of *Scorpio*.

I have recently become acquainted with some undescribed structures in *Limulus*, which appear to me to be strongly confirmatory of the identification of the structural elements of that animal with the structural elements of *Scorpio;* and, pending the preparation of a more complete account of the matter, I am anxious to place on record the chief grounds which appear to me to exist for classifying *Limulus* with the Arachnida in very close association with the pulmonate members of that class.

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* "Notes on Embryology and Classification." "Quart. Journ. Micr. Science," October, 1877.

Without depreciation of the morphological insight of those who have previously approached this question, amongst the foremost of whom are the English zoologists, Huxley, Woodward, and Owen, I may venture to say that it appears to me that the fact that *Limulus* is little else than an aquatic scorpion, and *Scorpio* little else than a terrestrial king crab, has not as yet become an accepted conclusion of zoological science, because those zoologists who have discussed the question and instituted a comparison between *Limulus* and *Scorpio*—whether (as Straus Dürckheim) favourable to their association or (as every other zoologist) opposed to it—have not to begin with hit upon the true terms of comparison. We have in *Limulus*, on the one hand, and in *Scorpio*, on the other, a series of more or less completely expressed body-segments and a corresponding series of appendages. Everything depends on making a correct start in the comparison, supposing that a very close agreement exists between the two series in a barely masked condition, and is to be made evident as the result of the comparison.

The notions as to the importance of the innervation of appendages from a ganglion lying in front of the mouth as opposed to the condition when innervation proceeds from a ganglion behind the mouth, have tended to obscure the true relationships of *Limulus* and *Scorpio*. For it is only quite recently that it has been established (Alphonse Milne-Edwards) that the ganglion in front of the mouth does *not* innervate the first pair of appendages of *Limulus*,* and that, similarly, the innervation of the first pair of appendages (cheliceræ) of Arachnida does not proceed from the primitive "ganglion in front of the mouth"; but from right and left lateral nerve-centres, which are embryologically quite distinct from the primitive præoral ganglion, and posterior to it (Balfour).

Whilst there has been a difficulty in regard to this point of comparison, which is now removed, there has not been a full and well-grounded conviction of the identity in structure and position of the series of projecting books of (about) 150 double lamellæ seen in *Limulus*, with the similar series of sunken books of (about) 130 double lamellæ, seen in *Scorpio*.

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* [June 24th, 1881. I am able fully to confirm the exactness of the account of the nervous system of *Limulus* given by M. Alphonse Milne-Edwards, having, through the kindness of Mr. Carrington, F.L.S., received from the Royal Westminster Aquarium a living specimen of *Limulus*, which enabled me to dissect out the relations of the nerves to the ganglionic centres in the most satisfactory manner.]

A careful study of the structure of these respiratory organs in the two animals, together with the explanation afforded by their external position in *Limulus*, of the "comb-like organs" of the Scorpion, and the observation of the fact that the genital operculum of *Limulus* is represented in miniature by the genital operculum of the Scorpion, and in an exactly corresponding position relatively to legs and gill-books, are the factors which, above all others, are likely to contribute to forming in an observer the impression that there is a very real and intimate agreement between *Limulus* and *Scorpio*. This impression is confirmed in a remarkable way by the agreement of details of structure in the two animals—such, for instance, as the form of the legs, the relation of the coxæ to the mouth, and to the sternal sclerites, the minute structure of the gill-plates, and their exact arrangement and imbrication to form those peculiar book-like organs which have no parallel in the animal kingdom. The impression is further confirmed by one of the main facts on which Straus Dürckheim relied for his conclusions on the subject, viz., the existence in *Limulus* and in the Arachnida of a very peculiar loose skeletal piece—the "entosternite": further by the close similarity of the digestive canal and its cæca in Limulus and Scorpio, by the close agreement of the very highly developed vascular system in the two animals; by the close agreement in its general disposition of the central nervous system in the two animals. In addition to all this, it has recently been shown that the structure of the compound eye of *Limulus*, though necessarily not that of any living Arachnid, is also unlike that of any living Crustacean (Grenacher). Lastly may be mentioned, as confirmatory of the existence of a close relationship between *Limulus* and the pulmonate Arachnida, on the one hand the living Arachnida, which, like *Limulus*, exhibit a suppression of the segmentation of the hinder part of the abdomen as compared with *Scorpio*, and on the other hand those fossil forms, the Eurypterina, which correspond with *Scorpio* exactly in the number of segments developed in the abdominal region, and very closely in their general form, whilst unmistakably related in the closest way to *Limulus* (as generally admitted) by the structure of their great digging leg (the hindmost pair) and of their genital operculum. The cephalo-thoracic limbs of the Eurypterina, though but five pairs in number in place of the six pairs present in *Limulus* and Scorpio, yet, as long since pointed out by Professor Huxley, present no difficulty on this account in the way of a close approximation of the forms in question; for it is obvious from the condition of the anterior pair in such a Eurypterine as *Slimonia*, that there is a *characteristic tendency to the reduction and hence suppression of the* *anterior appendages*, which has, in the case of the whole group of Eurypterina, led to the suppression of the foremost pair of appendages corresponding to the cheliceræ of the Scorpion and of *Limulus*. The probability of such a suppression having taken place has a direct bearing upon questions which arise when a comparison is attempted between the series of appendages of the Arachnida as a class with those of other classes of Arthropoda. It is significant in this connexion that such a suppression of at least one pair of anterior appendages has been very generally considered to be a characteristic of the Arachnida, whether the attempt to harmonise their structure with that of other Arthropoda has been made in assimilating them through *Galeodes* to the Hexapod Insects, or through *Scorpio* to the Decapod Crustaceans.

I now submit as a summary of the conclusions to which I am led, a tabular view of the structural identities of *Limulus* and *Scorpio*, in so far as the sclerites of the body and appendages are concerned; and shall next describe certain cavities and their stigmata which I have detected in *Limulus* which appear to me to be the representatives of the stigmata of the pulmonate Arachnida, and of the pulmonary chambers in which their gill-books have become concealed.*

Notes in reference to the Tabular Statement.

Camerostome.—The tubercular sclerite at the base of the first pair of appendages overhanging the mouth may be spoken of by this term (introduced by Latreille) both in *Limulus* and the other Arachnida, without prejudging the question as to its relations to the "labrum" of Hexapod Insects or of Crustacea.

Chilaria.—This term has been applied by Professor Owen to the pair of small processes placed mesially between the coxæ of the last pair of legs in *Limulus*. Although supplied by distinct nerves, yet their late appearance in development, as shown by Packard, and their simple structure, lead to the conclusion that they are not rudimentary appendages, as has been supposed, nor parts of the coxæ of the sixth pair of legs as has been suggested, but special tubercular elevations of the sternal surface.

Genital operculum.—On account of the concentration of the nerve-centres in the cephalo-thoracic region, and the consequent origin of the pair of nerves to this organ from that region, the genital operculum *Limulus* of has been regarded by some writers as belonging to the *cephalo-thoracic* tergites. It has, however, a distinctly emarginated tergal surface corresponding to it in the *abdominal* carapace. This emarginated area is much more distinct in the embryonic condition than in the adult. In *Limulus*, the median portion of the base of the genital operculum and of the following five lamelliform organs appears in each case to be a projecting ridge of the sternal surface simply, whilst the distal and lateral area only (right and left of the shallow median fissure) are to be regarded as representing a right and left "appendage." The exact limit of the two elements, that is, of the appendage proper and of its sternal base, can only be decided by an exact knowledge of the development of the parts in the embryo.

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* [June 24th, 1881. In October, 1878 ("Quart. Journ. Micr. Science") I published an account of the motility of the spermatozooids of Limulus. This character tends to separate Limulus from Crustacea. It also tends to unite *Scorpio* and *Limulus*. The most important difference between the two animals, is the absence from *Limulus* of Malpighian tubes. Rudiments of these may yet be discovered.]

Abdominal Carapace and Telson.—In young embryos of *Limulus* there are, according to Packard's observations, eight, if not nine, distinct segments indicated in the tergum of the abdominal region, besides the rudiment of the post-anal spine. Of these the first represents the tergite of the genital operculum, and, in the adult, is indicated by an emarginated area and *immovable* spine or angular process on either side of the fused mass forming the abdominal carapace. The next five embryonic segments correspond to the five pairs of appendages carrying the "biblioid" gills, and are indicated each in the adult by a movable spine, at the side of the tergum, and by five areæ on its free or pleural lower surface, marked out by the disposition of the hairs which clothe it.

The next following embryonic segment (the seventh) is indicated in the adult by a pair of lateral movable spines on the tergum (the sixth pair of movable spines), but there is no further indication of segments; what was seventh, eighth, and ninth segments in the embryo is fused into one piece, together with the segments in front, and exhibits no rudiment of the former segmentation in the form of ridges as do the first six abdominal segments. It forms an oblong area of considerable bulk, not much smaller in its axial portion than the whole of the abdominal segments in front of it. In the soft integument beyond this is placed the anus and beyond that the anal spine.

The word "telson" has been applied, in my opinion, unwisely to the post-anal spine. The telson of such a Crustacean as *Astacus* is that region of the body following upon the segmented region and carrying within its area the anus. It is (as more especially pointed out by Claus) a region which may be characterised as one of *potential* segmentation; it is not precisely a segment, but something more than a segment, viz., that *tract* of the hindmost region of the body which can, and in incompletely developed forms does, give rise to new segments, by a process of separation from its anterior border. It has necessarily, and by definition, a certain extension anteriorly, that is, in front of the anus.

Now, the post-anal spine of *Limulus* has no such character. It is homologous with the post-anal sting-spine of Scorpio, or the jointed post-anal filament of *Thelyphonus*, and if it may be compared with anything in *Astacus* it is with the post-anal joint of that Crustacean's telson.

The true "telson" of *Limulus* embraces the larger part of the unsegmented area, the area of potential segmentation which extends from the sixth pair of movable lateral spines and the axial area from the base of the last pair of gill-plates backwards. In the embryo this area has one or possibly two segments marked off from it, so that the small area formed by their re-fusion in the adult is, strictly speaking, not "telson."

The whole præ-anal region of the telson of *Limulus* (together with the two embryonic segments of which all trace is lost in the adult) corresponds to the six proximal (proximal that is to the anus) præ-anal body-rings of *Scorpio*, of which that including the anus should be called "telson." That this bulky region should remain unsegmented in *Limulus*, whilst giving rise to several segments (some elongate and annular) in *Scorpio*, is no difficulty in the way of the assimilation of the two forms, so far as Arachnidan characters are concerned, firstly, because in the Spiders, which are typical Arachnida, the abdomen is obscurely segmented in a manner parallel to that observed in *Limulus;* and, secondly, because in the Eurypterina (the close affinity of which to *Limulus* we are permitted to assume as demonstrated) the exact number of segments and an approximation to the form, shown by *Scorpio* in this part of the body, is found.

Whilst the term telson had, it seems, best be reserved for a larger area than that occupied by the post-anal spine, it is to be noted that Professor Owen has urged, not by the mere implication of a name, but explicitly, that the post-anal spine of *Limulus* corresponds to the several præ-anal segments of the Scorpion's tail as well as to its "sting," and he has pointed to indications of annulation in the structure of this spine. The traces of annulation appear to me to be precisely similar in character to the annulations of the jointed post-anal filament of *Thelyphonus*, and it appears to me that there is the same inducement when the comparison of general form only is made in the case of *Thelyphonus* compared with *Scorpio*, to regard the narrow-jointed post-anal "tail" of the former as the equivalent of the latter's tail, consisting of true præ-anal body-segments, as there is when *Limulus* and *Scorpio* are compared, to assimilate the king crab's spine to the segments of *Scorpio* just named. In yielding to the inducement in either case, we should, I think, mistake a resemblance of superficial adaptation of form for one of true genetic equivalence.

II.—THE PARA-BRANCHIAL STIGMATA OF LIMULUS.

The views which have been advanced in the preceding section of this note, as to the practical identity of the gill-books of *Limulus* and the lung-books of *Scorpio*, implicitly contain the affirmation that either the structures of *Limulus* have been derived from those of *Scorpio*, or those of *Scorpio* from those of *Limulus*, or that a third (possibly now extinct) form has given rise to both *Limulus* and *Scorpio*. Further, it is to be observed that such extinct form might be more like to *Limulus* than to *Scorpio*, or *vice versâ*, in respect of any particular element of structure.

To make a long story as short as possible, I may say that without prejudicing the recognition of the (as I think) well-established morphological identities above pointed out, we may best explain their existence by assuming that an aquatic form breathing the dissolved oxygen of the water inhabited by it, by means of book-like gills, was the common ancestor of *Limulus* and of *Scorpio*. From the book-like gills of this ancestral form, the broad series of *Limulus* and the narrower lung-books as well as the pectinæ or combs of the Scorpion, have been derived. The form of the book-like gills of this Arachnidan ancestor was probably something intermediate between the three existent modifications of it—and best conceived of, perhaps, by imagining the teeth of the Scorpion's "pectinate organs" to become soft and flattened and increased in number.

To obtain from these the *Limulus* gill, we have but to suppose certain definite changes of dimension, the imbrication and character of the lamellæ and their external position remaining unaltered.

To arrive at the book-lungs of the Scorpion, we have to imagine the ventral surface on each side in close proximity to the short appendages carrying the gill-books—to have become deeply cupped or depressed; so that two series of cup-like pits should be formed, a right and a left, a pair being placed in each segment, corresponding to each pair of gill-books. Each cup must have become so large in area and so deep as to embrace within its limits the relatively small adjacent gill-book. Further, when once the gill-book had been involved in this cup-like depression, the walls of the cup must have tended to grow together so as to form a pulmonary chamber with only a narrow slit-like opening to the exterior, and *pari passu* with this closing in of the cupped area—and the protection of the respiratory lamellæ—the Arachnid must have acquired the power of leaving the water and of breathing the atmospheric oxygen admitted to the damp chamber formed by the cave-like areas of depression.

Whilst framing such a hypothetical account of the way in which the transition from naked "gill-book" to in-sunken "lung-book" could have taken place, one naturally asks—"Is it not a somewhat gratuitous assumption to imagine that cupped areæ should form conveniently by the side of the gill-books of the aquatic ancestor, so as to be ready to increase in size and ultimately draw into themselves, as it were, the gill-books?" "Is there," we are led on further to ask, "any known instance in Arachnida of the formation of cupped areæ on the chitinous surface of the body? If so, can we show in what mechanical relation they are formed? And lastly, can it be demonstrated that such mechanical relation probably existed in connexion with the gill-books of the assumed common ancestor of *Limulus* and *Scorpio?*" If all these questions can be affirmatively answered, then our hypothesis as to the transition of the aquatic Arachnid to the pulmonate condition acquires great plausibility.

The answer to these questions, which I am able to give as the result of some careful examinations of specimens of *Limulus* and *Scorpio*, *Thelyphonus*, and other Arachnida, appears to me to have more than ordinary interest, since the formation of cupped areæ on the chitinous surface of the body and the mechanical relations connected with their formation have come to light as demanded by the hypothesis. They exist in *Limulus* itself and in *Thelyphonus*. In *Limulus*, there are two great muscles, a right and a left, inserted into the soft ventral integument near the base of each double gill-plate. These muscles known as the thoraco-abdominal muscles, serve (together with others which enter the appendage itself) by their contractions to move the gill-plates in the water and so aid in aquatic respiration. The position of the insertion of each muscular mass is marked by a deep funnel-like depression of the integument. From the external surface this depression appears as a "stigma," and a rod can be introduced into it to the depth of an inch. The funnel-like depression has a narrow mouth which is often as much as half an inch in length. Internally the invaginated cuticle stands up as a flexible chitinous tendon, giving attachment to the muscle already mentioned. These structures appear to have remained hitherto undescribed.

In *Limulus*, I find a pair of these "muscle-stigmata," right and left behind the genital operculum, and a pair (right and left) behind each of the lamelliform fused appendages which carry the gill-books. The mouth of each stigma is strengthened by a small "epistigmatic sclerite" and two adjacent still smaller sclerites.

We have only to suppose the appendages carrying the gill-books *not* to have fused as yet in the middle line, and the muscular stigmata to have become greatly developed (perhaps by increased development of the muscle aiding in aquatic respiration) and we have at once the gill-book sinking within the area of the stigmatic pit.

A very important feature in the supposed further development is the correspondence of the atrophy of the muscle (which atrophy is required to fit in with our hypothesis, and to convert the muscle-pit into a pulmonary sac) with the changes in the structures which would necessarily result were the physiological conditions gradually to become such as to favour aërial in place of aquatic respiration. The

violent agitation of the gills by the muscle attached to the stigmatic pit would become useless, supposing an exposure of the gill-lamellæ to the atmosphere became by degrees habitual with the ancestral Arachnidan. In proportion as these hypothetical creatures acquired the habit of aërial respiration—the deepening and arching in of the stigmatic pit would be favoured, and the atrophy and final disappearance of the muscle which was attached to its inner surface, and had mechanically brought it into existence, would also be directly promoted.

A further confirmation of the view now advanced is found in the remarkable Javanese Arachnidan *Thelyphonus*. This Arachnid has not four pairs of lung-sacs like Scorpio, but only two pairs, corresponding to the two foremost lungs of *Scorpio*, and to the second and third gill-book-pairs of *Limulus*. Nevertheless, the four segments of the abdomen posterior to these are each marked by a pair of shallow stigmata placed in line with the orifices of the pulmonary sacs of the two anterior segments. When the internal structure corresponding to these parts is examined, it is found that a large muscle (similar to the similarly placed muscle of *Limulus*) is inserted into each of the four right and four left stigmata in the segments posterior to the pulmonary sacs. The two segments into which the two pairs of pulmonary sacs are sunk, have no such muscles. The pulmonary sacs are, therefore, in this case, also, to all appearance, enlarged muscular stigmata, from which their former muscles have disappeared by disuse and atrophy.

XI. "Effects of Stress on the Thermoelectric Quality of Metals. Part I." By J. A. Ewing, B.Sc., F.R.S.E., Professor of Mechanical Engineering in the University of Tokio, Japan. Communicated by Professor Fleeming Jenkin, F.R.S. Received May 31, 1881.

(Abstract.)

This investigation was undertaken, in the first instance, with the view of finding whether the gradual mechanical change which goes on with lapse of time after stretching wires (which the author described in a former paper, "Proc. Roy. Soc.," vol. 30, p. 510) is associated with a corresponding change in thermoelectric quality. But the experiments brought to light some very unexpected results with regard to the *immediate* effects of stress, and the present communication deals with these only. Part I is further limited to the effects of longitudinal pull on iron. The author is proceeding to extend the same inquiry to other metals and to other modes of stress.

The method consisted in applying load to wires by the weight of a hanging tank containing water, which could be run in and run out at any desired rate, the junction between the stressed and unstressed parts of the wire being kept at 100° C. and 18° C. approximately. The thermoelectric effects were measured by the deflections produced in a short-coil mirror galvanometer, adjusted for great sensibility.

The effect of applying a moderate amount of longitudinal pull to iron wires was found to be negative (that is to say, the position of iron in the thermoelectric series, with the given condition of temperature, was shifted towards antimony). This result was merely a confirmation of the observations of Sir William Thomson ("On the Electrodynamic Qualities of Metals," "Phil. Trans.," 1856). But, instead of increasing up to the breaking point, the effect reached a negative maximum, after which it decreased and sometimes even changed to positive before the wire broke. This result was confirmed by a large number of tests of different wires.

When loading was stopped shortly before the wire broke, and the load was gradually withdrawn, the thermoelectric effect passed back through a negative maximum, different from the first, and, finally, with no load, it reached a positive value as a consequence of the stretching which had taken place. (This positive effect due to previous stretching when the load was withdrawn had been observed before by Magnus and by Thomson.) The fact that the thermoelectric quality passed back through a negative maximum during unloading showed that the maximum which had occurred during loading was not the result of an antagonism between the influences of stress and strain.

A second loading of the same wire gave a series of thermoelectric values greatly different from those got in either of the two former processes of first loading and unloading, and showing another negative maximum long before the new limit of elasticity was reached. When that limit was passed, the subsequent drawing out of the wire was associated with a relatively rapid change in the thermoelectric quality towards positive.

When, after a wire had once been stretched, any given load was gradually applied and removed successively within the new elastic limit, the thermoelectric effects for equal intermediate amounts of stress during loading and during unloading were widely different, but passed through a cyclic series of values for each repetition of the cyclic change of stress. This effect is shown in the paper by curves which give the relation of stress to thermoelectric current. The curves got by putting on load (not exceeding the elastic limit) and by taking it off are far from coincident, but form a closed figure containing a wide area.

This cyclic phenomenon is experimentally studied at length in the paper, and the thermoelectric effects of various cycles of loads are shown graphically in a number of diagrams. To apply a load, remove part of it, re-apply it, and continue the application, forms a wide but closed, or nearly closed, loop on the " on " curve. Similarly, to remove load, stop during the removal, and replace part, and then go on again with the removal, forms a loop of a like kind on the " off " curve.

The curves got during the gradual removal of load always show a negative maximum, even though the applied load has been less than that required to pass the negative maximum during loading.

The effect of beginning to reload a stretched wire which has been unloaded is at first feebly positive, passes a positive maximum, becomes negative, passes a negative maximum, and finally, if the wire does not break too soon, becomes once more positive before rupture takes place.

The leading characteristics of the cyclic action mentioned above are sufficiently obvious from the diagrams, but can scarcely be described verbally, except at great length. The curves show a lagging of thermoelectric effect when the stress is changed. It is proved that this lagging of effect is not a time action, for it is shown to be independent of the rate of increment and decrement of the stress. Moreover, the thermoelectric value associated with any load (not exceeding the elastic limit), arrived at in any manner, is constant so long as the load is constant.

One very remarkable feature of the curves is that *the first effect of reversal from loading to unloading, or from unloading to loading, is to continue the same kind of thermoelectric change which has been going on just before the reversal takes place.* For example, if the wire had been becoming negative when loading was stopped and unloading began, it continued to change towards negative during the very first part of the subsequent unloading. This law appears to be general.

As a consequence of the cyclic action it follows that there may exist between a stress and its associated thermoelectric effect any relation which can be expressed by a point within the wide area enclosed between the most distant " on " and " off " curves. To attempt, therefore, to assign a relation irrespective of the preceding states and changes of stress would be altogether futile.

Instances are given of "molecular reminiscences" of previous stress-actions, exhibiting themselves by modifying the form of the curves in the next succeeding experiments with the same wire.

It is shown that *mechanical vibration greatly reduces, if it does not wholly destroy, the distinction between the " on " and " off " curves of thermoelectric quality and stress*, if the wire be kept in a state of vibration during the application and removal of the load.

It is also shown that two widely different values of thermoelectric quality, got by reaching the same stress in two different ways (namely by addition of load from zero and by partial removal of load from a high value), become almost equal to each other when the wire is vibrated, not during, but after, the changes of load through which the given state has been reached.

It is also shown that this approximate equality produced by vibration continues after the vibration ceases. Also, that when a cycle of loads is gone through afterwards, without vibration, the old difference between the "on" and "off" curves reasserts itself.

It is suggested that the cyclic phenomenon so conspicuous in this investigation is not peculiar to the thermoelectric effects of stress, but is probably present in other effects of stress, and may perhaps be found to occur in the changes of any quality of matter which is a function of another variable quality (such as temperature) when the latter quality is subjected to increment and decrement.

Lastly the results of certain independent experiments made by others in other branches of physics are referred to in confirmation of this suggestion.

XII. "On the Reversal of the Lines of Metallic Vapours. No. VIII. (Iron, Titanium, Chromium, and Aluminium.)" By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received June 2, 1881.

In our last communication on this subject we observed ("Proc. Roy. Soc.," vol. 29, p. 405) that iron introduced as metal, or as chloride, into the electric arc, in a lime crucible, in the way which had proved successful in the case of many other metals, gave us no reversals. We succeeded, however, in reversing some ten of the brightest lines of iron, mostly in the blue and violet, by passing an iron wire through one of the carbons, so as to keep up a constant supply of iron in the arc. Considering the great number of iron lines, and that so many of them are strongly represented amongst the Fraunhofer lines, it seemed somewhat surprising that it should be difficult to obtain a reversing layer of iron vapour in the arc inclosed as we use it in an intensely heated crucible. A like remark might be made respecting titanium, which is almost as well represented as iron in the Fraunhofer lines, but has heretofore given us no reversals. Almost the same might be said of chromium, except that the number of chromium lines is so much less than that of either of the other two metals.

We have since found that most, if not all, of the strong lines of these three metals may be reversed by proper management of the atmosphere and supply of metal in the crucible. Indeed, with regard to iron we have found that the method employed with other metals was successful so far as the ultra-violet rays were concerned, though it failed for less refrangible rays. When iron has been put into the crucible through which the arc of a Siemens' dynamo-electric machine is passing, and then fragments of magnesium dropped in from time to time, most of the strong ultra-violet lines of iron are reversed. The magnesium seems to supply a highly reducing atmosphere, and to some extent carry with it the iron vapour. It also produces a good deal of continuous spectrum, at least in certain regions, and against this the iron lines are often depicted on the photographic plates sharply reversed. In this way we have observed the reversal of the strong iron lines about the solar lines L and M, four strong lines below N, the line O, all the strong lines from S_2 to U inclusive, and two strong groups still more refrangible.

Potassium ferrocyanide introduced into the arc instead of magnesium gives a reversal of the same lines as are mentioned in the foregoing paragraph.

Iron wire fed in through a perforated pole gives reversals of the highest group (wave-length 2492 to 2480), but with the lines so much expanded as to form broad absorption-bands instead of lines.

With a vertical arrangement of the carbons, similar to that used by us for resolving the fine double lines (" Proc. Roy. Soc.," vol. 32, p. 231), and a stout iron wire in the axis of the lower (positive) carbon, many more lines in the visible part of the spectrum are seen expanded and reversed. This effect is sometimes enhanced by leading into the crucible through the upper carbon, which is perforated for the purpose, a very gentle stream of hydrogen gas; the stream must be no more than is just sufficient to give a tiny flame at the mouth of the crucible; a stronger stream diminishes the amount of metallic vapour, probably by its cooling action, and lessens the effect. By this treatment, some of the strongest lines of iron remain reversed for some time, the weaker lines are seen to expand and be reversed for a few seconds at a time, when, from a change in the intensity of the current, or some other reason, a larger amount of metal is volatilized and shows itself by burning in brilliant scintillations at the mouth of the crucible.

A list of the iron lines reversed, designated by their approximate wave-lengths, is subjoined.

5614·5
5585·5
5445·6
5428·5
5405
5396
5370·5
5327 the more refrangible of this pair.
5268·5 E

5226 the less refrangible of this double line.
5166·8 b_4
5064
5072
4956·5 both lines of pair, the less refrangible strongly.
4919·7
4918
4890·5
4890
4871 both lines of pair.
4414·5
4404
4382·5
4325
4314·5
4307 G
4271 both lines of pair.
4071
4063
4045
3898
3886
3859
3856
3833·5
3827·5
3825
3824
3820 L
3815
3767
3763·5
3757·5
3745·5
3736·5
3734·5
3727 M
3722
3720
3707·5
3705·5
3647
3631
3502
3492
3475
3465·5
3440 O
3402·5
3272*
3246*
3099·5 S_2
3082
3072
3057
3056
3046·5 *s*
3041
3036·5
3030
3024·5
3022·5
3020 T
3017·5
3016·5
3008·5
3007·5
3006·5
3000
2999
2994 *t*
2984
2982
2974
2971
2967·5
2965·5
2957
2953·5
2948 U
2941
2937
2932
2929
2912
2871
2866·3
2860·8
2836·4
2829·8
2824·2
2767·3
2755·2
2754·3
2749
2748
2744·7
2743
2741·3
2736·2
2722·7
2720
2718
2549·5
2545·5
2541
2535·7
2534
2529
2528·6
2527·3
2524·2
2523
2518·3
2511
2502
2492
2491·6
2491
2489·3
2485·4
2484·6
2481
2480
2474
2465

It is by no means always the strongest lines which are reversed. For instance, in the group of lines between wave-lengths 5428·5 and 5396 there are four strong lines 5423·5, 5414·6, 5403, and 5382·4, which are all much expanded and diffuse when iron is introduced into the arc, but have not been seen reversed by us, while three other lines —5396, 5405, and 5428·5—are well reversed, but not much expanded.

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* It is possible that these may have been the copper lines which were reversed, and not the iron lines, which are nearly identical in position.

Considering the enormous complexity of the iron spectrum, which probably reaches to something like a thousand distinct vibrations, we should expect to find a greater variation in the relative strength and reversibility of the lines of this metal, according to the circumstances in which it is placed, than we find in the case of metals giving simpler spectra. So that we may say, on the whole, that the phenomena in the case of iron are of much the same general character as we have observed before in the case of other metals; but the number of observations to be made on so extensive a subject renders it extremely difficult to come to any general conclusions.

When the perforation of the lower carbon is filled with titanium cyanide instead of the iron wire, the titanium lines come out very brilliantly and steadily, and many of them, especially in the green and blue parts of the spectrum, are expanded and reversed. We give a list of those we have observed to be reversed.

6260
6257 both lines of pair.
5223
5209
5192
5129
5064
5039
5038
5036
5035
5013
5007
4999
4990
4981
4535·5
4535
4533·2
4531·7
4690·5
4681
4666·5
4655·5
4617
4305
4299·5
4299
4298
4295
4290·7

In the case of chromium, introduced into the crucible either as oxide or as bichromate of ammonia, there were no reversals until a gentle current of hydrogen or of coal gas was led in through the perforated carbon. This brought out the triplet in the green, wave-lengths 5207, 5205, 5203, sharply and steadily reversed, and likewise the three strong lines in the indigo, wave-lengths 4289, 4274, 4253; also a triplet near N at wave-lengths about 3578, 3593, 3606, apparently coincident with strong lines in Cornu's map of that part of the solar spectrum, and a rather strong double line just below O at about wave-length 3446. The reversal of another chromium line at about wave-length 3217 is doubtful. A triplet at wave-lengths 2799·8, 2797, 2794, is more easily reversed than any other of the chromium lines. This triplet is generally strongly developed whenever a compound of chromium is introduced into the crucible, so that we conclude that it is due to that metal, but it is sometimes visible in the photographs when other chromium lines are not seen. A still more refrangible chromium line, wave-length about 2779·6, is also frequently reversed by a gentle current of hydrogen.

The two aluminium lines near S are frequently reversed when a fragment of the metal is dropped into the crucible, the less refrangible line, wave-length 3091·5, being more strongly reversed, and continuing reversed for a longer time than that at wave-length 3080·5.

XIII. "Note on a Comparison of the Diurnal Ranges of Magnetic Declination at Toronto and Kew." By BALFOUR STEWART, LL.D., F.R.S., and WILLIAM DODGSON. Received June 9, 1881.

Through the kindness of the Science and Art Department, South Kensington, and of Mr. Carpmael, Director of the Toronto Observatory, we have received daily values (excluding Sundays) of the diurnal range of magnetic declination at Toronto. These observations extend from 1856 to 1879 inclusive, thus forming a series of twenty-four years. Each number is the difference in scale divisions of the declinometer between the greatest eastern and greatest western deflection of the declination magnet on each day, as observed at the hours 6 A.M., 8 A.M., 2 P.M., 4 P.M., 10 P.M., and midnight, of Toronto mean time, one scale division of the instrument being equal to 0′·72 nearly. It is probable that such differences represent very nearly the true diurnal range.

Disturbances appear to be violent at Toronto, and we have rejected a few of the most disturbed observations, embracing those which denote ranges above forty scale divisions, or 28′·8. In all 107 observations were thus rejected in the series of twenty-four years. Although this rejection has been made, it must not be supposed that the remainder are entirely undisturbed, but are only freed from the excessive influence of a few violent disturbances. We have reduced in the meantime the sixteen years extending from 1858 to 1873, in order to compare them with a similar series of the Kew diurnal declination ranges, including disturbances.

The method of reduction has been precisely that adopted for the Kew series, and already described by us (see "Proc. Roy. Soc.," No. 20, 1879, p. 313).

Our object was to see whether there is any difference in phase between the various Toronto and Kew inequalties, and for this purpose we have adopted precisely the method formerly pursued by us when comparing together the declination ranges at Kew and Prague (see "Proc. Roy. Soc.," vol. 29, p. 316).

We have thus obtained the following result:—

Table 1.—Algebraic sum of Toronto and Kew Magnetic Inequalities.

Toronto and Kew (Toronto 1 division to left) = 154514.
„ (together) = 161094.
„ (Toronto 1 division to right) = 165016.
„ „ 2 „ = 165418.
„ „ 3 „ = 161404.
„ „ 4 „ = 154320.

It would appear from this table that the phases of the various magnetic inequalities occur at Toronto nearly two days (more strictly 1·6 days) before the advent of the corresponding phases at Kew.

The result already obtained for Kew and Prague shows that the phases of magnetic inequalities occur at Kew nearly one day (more strictly 0·7 day) before the advent of the corresponding phases at Prague. Thus the two results agree together in representing a progress of magnetic weather from west to east, and agree also with a result obtained by Balfour Stewart and Morisabro Hiraoka (see "Proc. Roy. Soc.," vol. 28, p. 288), showing that magnetic weather changes occur at Trevandrum, in India, 9·7 days later than at Kew.

It ought, however, to be borne in mind that in the intercomparison of Toronto, Kew, and Prague, the observations include disturbances, while in the intercomparison of Kew and Trevandrum the undisturbed observations at Kew are compared with the whole body of observations at Trevandrum, this latter being a tropical station in which the effect of disturbance is extremely small.

XIV. "On the Absorption of Gases by Solids." By J. B. HANNAY, F.R.S.E., F.C.S. Communicated by Professor G. G. STOKES, D.C.L., &c., Sec. R.S. Received June 4, 1881.

During the progress of the investigations which I have from time to time had the honour of bringing under the notice of the Royal Society, I have again and again noticed the apparent disappearance of gases inclosed in vessels of various materials when the disappearance could not be accounted for upon the assumption of ordinary leakage. After a careful examination of the subject I found that the solids absorbed or dissolved the gases, giving rise to a striking example of the fixation of a gas in a solid without chemical action.

In carrying out that most troublesome investigation, the crystalline separation of carbon from its compounds, the tubes used for experiment have been in nine cases out of ten found to be empty on opening them, and in most cases a careful testing by hydraulic press showed no leakage. The gases seemed to go through the solid iron, although it was 2 inches thick. A series of experiments with various linings were tried. The tube was electro-plated with copper, silver, and gold, but with no greater success. Siliceous linings were tried—fusible enamels and glass—but still the tubes refused to hold the contents. Out of thirty-four experiments made since my last results were published, only four contained any liquid or condensed gaseous matter after the furnacing. I became convinced that the solid matter at the very high pressure and temperature used must be pervious to gases.

This I find to be the case, and to a very remarkable extent. I am still investigating this matter, but as I have so much on hand it may be some time before I can finish the work, so I wish to place on record the results so far as I have proceeded. I find that glass, when at a temperature of about 200°, absorbs a large quantity of gas when the latter is under a pressure of 200 atmospheres. Oxygen and carbon dioxide have been used, and have been found to be largely absorbed, and on cooling the glass under pressure the gas is retained permanently fixed. So much is absorbed that, on quickly raising the temperature to the softening temperature of the glass, the sudden escape of gas drives the glass into foam. On slowly raising the temperature and retaining it at 300°, most of the absorbed gas is given off without any visible action.

The frothing up of the glass by the outrush of gas is very striking. Other silicates, and also borates and phosphates, absorb gas, especially carbon dioxide, under great pressure. Metals absorb hydrogen and some of its compounds with carbon. As the treatment of quantities of matter sufficient for analysis at these high pressures and temperatures is a matter of great difficulty, the majority of experiments being failures, the work proceeds but slowly; but I hope during the summer and autumn to be able to elucidate the subject quantitatively, when I shall detail the results to the Society.

XV. "On the States of Matter." By J. B. HANNAY, F.R.S.E., F.C.S. Communicated by Professor G. G. STOKES, D.C.L., &c., Sec. R.S. Received June 4, 1881.

The conception which had been held from the earliest times that the three recognised states of matter were clearly separated from each other received a rude blow from the interpretation put upon the work of Andrews, that the liquid and gaseous states were really continuous, and that the two states could only be classified under one head—the fluid state. Andrews demonstrated that by placing a liquid under a pressure greater than the critical, and then raising the temperature, the liquid might be made to pass to an undoubtedly gaseous state without any sudden change having been visible. Thus the continuity of the liquid and gaseous states seemed established. I say seemed, because I have shown in former papers that under any pressure the fluid passes at a given temperature from a state where it possesses cohesion, capillarity, or surface tension—the distinguishing property of liquid, which prevents it freely mixing with a true gas—to a state where it possesses no cohesion, capillarity, nor surface tension, and where it mixes freely with any gas—in fact, to the gaseous state; and this change takes place at a fixed temperature independent of pressure.

As MM. Cailletet and Hautefeuille have recently come to the conclusion that the continuity claimed by Andrews does not exist, and have thus corroborated my work, I wish to place on record more fully the conclusions to which this work has led me.

In examining a subject of this kind it is important that we should first arrive at a clear understanding of the meaning of the language we use; second, of the means we have of proving our knowledge of the state of matter; and, third, of the value of such proof.

First, then, as to the meaning to be attached to the words solid, liquid, and gaseous. The solid state is that state of matter which in an isolated portion free to move displays sufficient rigidity of its substance to retain permanent irregularity of figure. The liquid state denotes that state of matter which in an isolated portion free to move displays sufficient freedom of motion in its substance to assume a form in accordance with the forces acting upon it, but which when partially filling an inclosed space retains a permanent limiting surface exhibiting capillarity. The gaseous state denotes that state of matter in which it assumes no surface nor definite figure, and which is so extensible that any quantity will distribute itself throughout a space.

Having defined the nomenclature, let us see what means we have of proving our knowledge of any state. Let us conceive that we are working with what we rarely or never have—pure substances. When water is nearly pure it freezes perfectly transparently; in fact, except for rigidity and difference in density it could not be distinguished from liquid water. Now consider a vessel frozen quite full of pure ice; there is no free surface to examine, therefore one test of its state is gone. Now let enormous pressure be brought to bear upon the ice till its density is reduced to that of water at the same temperature, and let the temperature be the new freezing-point; then there is no way of knowing whether the inclosed water is liquid or solid, without applying special tests. Either the vessel must be broken to test the rigidity of the contents or motion must be given to the vessel, and dynamic tests made as to the behaviour of the contents; but what I wish to impress is, that *sight* can teach us nothing directly as to the condition of the contents. A free surface is required to indicate the state of matter. So it is with the liquid and gaseous states, but just as they are more subtle, so are the modes of experimenting with them more difficult. Let us consider the case of a liquid inclosed in a space bounded by transparent sides having a piston free to move. When the pressure of the piston is less than that of the vapour of the liquid, there will be a space occupied by vapour over the liquid, and the surface of the latter will be visible and its state recognisable at a glance; but let there be more pressure upon the piston than equals that of the vapour, then no vapour will be formed, and the liquid will fill the whole cavity, in which state the whole substance cannot be determined by sight alone, because no free surface is visible. It might be solid for all our eyes could tell us. Here, again, we see that a free surface is necessary for determining the state. In Andrews' experiments there is no free surface, for at pressures greater than the critical a free surface is impossible. It was towards the procural of this free surface at any pressure that my work tended. The last definition is that of the phrase "free surface." A free surface is that surface of a solid or liquid matter which is in contact with vapour or gas, in fact, a surface in contact with a space where it has freedom of motion.

The difference between the solid and liquid states being easily recognisable, need not be stated, but that between the liquid and gaseous requires some consideration. Regnault has shown that every liquid has a certain molecular activity corresponding to each temperature, in virtue of which it throws off from its free surface with a definite mean velocity, a certain number of molecules per unit of surface. When the space over the liquid is limited, some molecules pass back again into the liquid, and when this number equals that of those outgoing, the space is said to be saturated. The pressure which the molecules by their impacts cause upon the sides of the vessel is called the vapour tension of the liquid. When the temperature rises, a greater number of molecules are thrown off than with the liquid, and the density of the vapour increases until it is nearly equal to that of the liquid. But still there is a limiting surface indicating the lower portion to be liquid. Let the temperature be now raised till the densities of the upper and lower portions are equal, then we have a state of matter in which the substance "assumes no surface nor definite figure, and which is so extensible, that any quantity will distribute itself throughout a space," as we before defined the gaseous state.

Now as to the proof that it is really in the gaseous state. The general effect of the attraction of the molecules of a liquid for each other and for solids, yields three measurable phenomena. First, the property of tensile strength measurable in liquid films (as soap-bubbles), second, capillary attraction, and third, the cohesion by reason of which the neck of a drop can support it as a wire supports a load. It has been shown by experiment, that all these three manifestations of attraction decrease as the temperature rises, and each may be used as a measurement of the other. As "surface tension" is *the* feature of the liquid state, it was necessary to investigate how it is affected by the gas or vapour with which it is in contact, and the results showed that the height of a liquid in a capillary tube is not perceptibly affected by replacing the air by hydrogen, or removing everything except the vapour of the liquid. Even when hydrogen was compressed at 200 atmospheres pressure over a liquid in which it is nearly insoluble, the capillarity only fell very slightly. It appeared that the cohesion or capillarity of a liquid depends upon temperature alone, and that pressure has no effect upon it.

Practically, it was found that the less soluble a gas is in a liquid the less effect does its presence cause at any pressure, and it is certain that had we a gas totally insoluble in a liquid, we might compress that gas under any pressure over the surface, without effecting the slightest change in the capillarity of the liquid.

To return to Andrews' experiment. It was seen that to determine the state of every quantity of matter, it was necessary to have a free surface, but by Andrews' mode of experimenting at pressures greater than the "critical," this was impossible, the tube being filled with a homogeneous fluid; but it is possible to have a free surface, and to watch the alteration of the capillarity at any pressure, provided a gas can be obtained which is insoluble in the liquid. We have seen that when a gas dissolves in a liquid, the capillarity of the mixture is lower than that of the liquid alone, so by these means we can tell whether or not a gas is soluble. In this way it was found that hydrogen was insoluble in several liquids, and thus the conditions were established for examining loss of capillarity with rise of temperature at any pressures, the hydrogen keeping a free surface without affecting the liquid. It was found that the capillarity fell to zero at the same temperature, independent of pressure; in other words, the matter passed from a state where it "retains a permanent limiting surface exhibiting capillarity," to a state where it "assumes no surface nor definite figure, and which is so extensible, that any quantity will distribute itself throughout a space," as we defined the liquid and gaseous states.

The state of affairs in those experiments is this:—The hydrogen takes the place of a portion of the vapour of the liquid, but unlike the molecules of the vapour, its molecules are unable to penetrate the surface of the liquid, in any quantity, but merely act by striking against the molecules which would otherwise pass out, and driving them back into the liquid. Could there be any greater proof than this of the special properties of a liquid surface? Let the temperature be very near the critical, yet the mixture over the liquid retains a very much lower density than the liquid, as the hydrogen does its work in keeping back a portion of the vapour molecules. In this case the upper does not become equal in density to the lower portion of the fluid as in Cagniard de la Tour's experiment, yet when the critical temperature is reached the meniscus disappears and diffusion occurs between the upper and lower portions. This phenomenon is seen at any pressure up to five times the "critical" pressure, showing that the change of state is independent of pressure. Gaseity is dependent upon molecular velocity (thermal activity), and to give it this velocity a very strong cohesion must be overcome. The velocity is not in any way dependent upon the pressure or upon the size of the vessel, and therefore the gaseous state proper cannot be altered by pressure alone. The liquid state is thus bounded by an isotherm which marks the thermal activity equivalent to the attraction of the molecules, and the continuity of the liquid and gaseous states ennunciated by Andrews is only apparent.

The passage from the liquid to the gaseous state might be represented by a curve where the line representing decrease of capillarity repasses through zero to a negative side, which in reality represents repulsion. In other words, the curve for liquid represents excess of attraction over repulsion, that for gas excess of repulsion over attraction.

The existence of this cohesion in liquids has been too much overlooked by the statistical method of looking at the states of matter. It is something very great compared with the amount of energy required to give the molecular velocity required to sustain the gaseous state. Let us by Dr. Joule's method make a calculation. Suppose the molecules in a gram of steam to be at rest it would require 92 gram-degrees, or, in absolute measure, 383,161 $\frac{\text{mtr.}^2\text{grm.}}{\text{sec.}^2}$, to give them velocity enough to exert a pressure of one atmosphere. Now the energy added to cause a gram of water to become steam at one atmosphere pressure is 540 gram-degrees (or 2,244,780 $\frac{\text{mtr.}^2\text{grm.}}{\text{sec.}^2}$), or about six times as much work to be done. And this is not all. We started from rest in the other calculation, here we start from water whose molecules are in motion. Five-sixths of the energy has gone to overcome the cohesion of the liquid. As we know, that to bring matter to the same state we must expend the same amount of energy, and as we may bring matter to the gaseous state either by boiling or by Andrews' apparently continuous method; in the first case we would say the heat used up represents the latent heat of steam, and in the other the specific heat of water, but they are both really measures of the thermal value of the energy required to overcome the attraction of the molecules. When, therefore, this energy has been given to the water, the cohesion has been overcome and the liquid state passed, whether or not it has been visible to our eyes. Thus from all evidence, experimental and theoretical, the liquid state has a limit, and the liquid and gaseous states are not really continuous.

The definition of the gaseous state as a state of matter not alterable by pressure alone leads us to a clear division of aeriform matter into two states, the vaporous and gaseous, the first alterable, the second unalterable by pressure alone. Another distinction between vapour and gas is this: gases are solvents of solids; vapours are not. Let a liquid be coloured by having some non-volatile coloured solid dissolved in it, and let it be heated under pressure, the liquid will remain coloured while the vapour will be quite colourless, and will remain so up to the critical point.

Now let the fluid be raised above its critical point, all the internal space will be coloured, showing that (the contents being gaseous) the gas dissolves the solid while the vapour does not. We have here a clear separation of the two kinds of aeriform fluids. The definition which I applied to the gaseous state at the beginning of this paper does not apply to the vaporous state, as we know that any quantity of it will not distribute itself throughout a space, because if we try to force vapour into a space already saturated, we cause a change of state, and a portion of the matter becomes liquid. Thus, instead of only two we have four distinct states of matter: solid, liquid, vaporous, and gaseous.

XVI. "The Relation of the White Blood Corpuscles to the Coagulation of the Blood." By L. C. WOOLDRIDGE, B.Sc. Lond., Physiologische Anstalt, Leipzig. Communicated by Dr. LAUDER BRUNTON, F.R.S. Received June 8, 1881.

(Abstract.)

The following is an abstract of some researches which have been carried out by the author in the Physiological Institute of the University of Leipzig.

It has long been known that the white blood corpuscles are concerned in the coagulation of the blood.

Alexander Schmidt, to whom we principally owe our knowledge of this fact, has distinctly formulated the part they play. He considers them as the source of two of the three factors which are, according to his well-known theory, necessary for the formation of fibrin.

The two components which arise from the white blood corpuscles are, according to Schmidt, paraglobulin and fibrin ferment.

The recent researches of Hammarsten have made it very probable that paraglobulin is not directly concerned in the formation of fibrin.

If this be true, and if the views of Schmidt concerning the participation of the white corpuscles be also correct, the latter must necessarily only play a very subordinate part; that is, they must be mere *ferment producers*.

In order to arrive at exact conclusions on this subject, the author has considered it necessary:—1st. To attain some more knowledge than we at present possess concerning the chemical nature of the white blood corpuscles. 2nd. To have exact data for the amount of white blood corpuscles which disappears during coagulation.

The first of these two subjects the author considers here.

For this purpose the leucocytes of lymphatic glands have been used as material.

It can be considered as in the highest degree probable that these are essentially identical with white blood corpuscles.

The leucocytes are obtained by finely dividing the glands (mostly the mesenteric glands of the calf) and gently rubbing the fragments in a mortar with ½ per cent. solution of common salt.

By filtration through a fine cloth, the cells are separated from the other constituents of the glands which remain behind on the filter, the cells passing through.

By means of the centrifugal machine, the leucocytes are well washed out with a ½ per cent. solution of common salt. The cells are perfectly distinguishable under the microscope and are apparently unchanged. After the washing out is completed, the cells are suspended in a little normal salt solution and are subjected to various experiments, the most important of which are as follows:—

1. *The lymph cells are changed by simple chemical reagents into a substance closely resembling fibrin.*

If to one volume of suspended cells an equal volume of 10 per cent. solution of common salt is added, the whole is immediately converted into a peculiar semi-transparent jelly.

If this be poured into water, or into 1 per cent. solution of common salt, it becomes immediately opaque.

It now appears in the form of a white rounded lump, or it forms large membranes, often many inches in extent.

These latter have the greatest resemblance to fibrin membranes such as appear in plasma obtained by various methods. The resemblance of the product to fibrin is made much more apparent if the substance be freed, by means of filtration and expression, as much as possible from water. It then appears as small flocculi, distinctly fibrous in their texture and elastic.

The chemical behaviour of these flocculi are as follows:—

In water they are insoluble.

In solutions of common salt, they gradually swell up.

In 0·2 per cent. hydrochloric acid, they are totally insoluble; if anything, they become firmer and more elastic.

In dilute alkalies, they gradually dissolve.

The microscopical examination shows that the cells, as such, have disappeared. Only nuclei, imbedded in a distinctly fibrous ground substance, are visible.

If the leucocytes are treated with distilled water, or with solutions of sulphate of magnesia, similar results are obtained. The cells are changed into a fibrous mass, with nuclei imbedded therein.

2. *Behaviour of Leucocytes towards Plasma.*

The only animals at the author's disposal were dogs. The most convenient way of obtaining plasma from dog's blood is to inject a solution of peptone into the blood of the living animal. If the animal be bled five or ten minutes after the injection of the peptone, the blood does not coagulate. (This fact was discovered by Dr. Adolf Schmidt, Mulheim.)

By means of the centrifugal machine the blood corpuscles can be separated from the plasma. With this plasma I have experimented, and I shall call it peptone plasma.

A general account of the action of peptone on the blood has been published by my friend, Dr. Fano. His researches were also carried out at Leipzig, and his observations have supplied me with many necessary details. My observations have led me to the conclusion that peptone plasma behaves towards the cells in a manner which is essentially similar to that in which solutions of common salt, of sulphate of magnesia, and distilled water behave. As I have already stated, by these latter reagents the protoplasm of the lymph cells becomes converted into a fibrous mass resembling fibrin.

When peptone plasma is the destroying agent, the substance produced is undoubtedly fibrin, and it owes its origin to a simple transformation of the protoplasm of the leucocytes into fibrin. It is perfectly independent of the presence of a fibrinogenic substance in the plasma.

The grounds on which I base this statement are clearly brought forward in the following experiments, which I shall adduce as examples.

A dog was injected with peptone; five minutes afterwards it was bled. The corpuscles were separated from the plasma by the centrifugal machine, and the plasma allowed to stand overnight in ice. During the night, incomplete coagulation of the plasma occurred.

This is a perfectly normal occurrence in peptone plasma, and it can be immediately brought about by dilution of the plasma with water, or by passing a stream of carbonic acid through it.

The plasma in question, after the coagula had been separated by filtration, presented the following characters :—

It was totally uncoagulable—

1. On dilution with water.
2. On passing a current of CO_2 through it.
3. On addition of Schmidt's fibrin ferment.
4. On addition of paraglobulin.
5. On addition of normal serum.*
6. On standing till it was foul.

In short, by no means could the presence of a coagulable substance—fibrinogen—in the plasma be demonstrated.

It behaved as follows towards cells:—

To 20 cub. centims. plasma a very small quantity of lymph cells were added. Two minutes afterwards it coagulated, but only imperfectly. The coagulum contracted rapidly. The serum which exuded was divided into two portions, to one a large quantity of lymph cells were added; very complete coagulation occurred in two or three minutes. The other portion showed not the slightest sign of coagulation for the twenty-four hours it was under observation.

This process was repeated four times; ultimately, the plasma lost the property of changing the cells into fibrin.

Now, I say the plasma changes the cells into fibrin, and I base this statement on the following facts, in addition to those already mentioned:—

1. The weight of the coagulum found is identical, that is, as nearly so as can be in such determinations, with the weight of cells which have been added.

2. The percentage of albumens in peptone plasma before coagulation with cells is identical with the percentage after coagulation with cells.

3. The protoplasm of the cells has completely disappeared and has been converted into a partly fibrous, partly granular, ground substance the nuclei remain.

4. If to a very large quantity of suspended cells, say 50 cub. centims., a very small quantity (1 cub. centim.) peptone plasma be added, the whole clots firmly. The microscope shows that the cell-body has disappeared.

These facts suffice to show that the plasma converts the cells into fibrin, and that this conversion is independent of the presence of a coagulable substance (fibrinogen) in the plasma.

But this is not all. I have already referred to the fact that a spontaneous coagulation occurs in peptone plasma on standing, and that this can be accelerated by dilution with water, or by passing a current of carbonic acid through it. Solutions of the fibrin ferment also bring it about, but not very much more rapidly than mere dilution.

In some cases, the coagulation which can be induced by these means is very complete, in others it is very scanty. The case I have described at length was one of the latter.

In other cases, the coagulation is very complete, a firm dense clot is formed by the means adopted. Now, on adding cells to such a plasma, not only are they converted into fibrin, but they induce the coagulation of the existing fibrinogen in the plasma.

I may here remark, that I have failed to find any ground whatever for the assumption that paraglobulin arises from lymph corpuscles.

In the destruction of the lymph cells by salt solutions the only product is the fibrinous body mentioned, not the slightest trace of a globulin can be detected.

In peptone plasma, paraglobulin is present in large quantities, and yet it is quite certain, as I shall mention later, that no breaking up of white blood corpuscles has taken place.

3. The generally received view, as to the course of events in the normal coagulation of the blood, is as follows:—

Very soon after leaving the body the white corpuscles die; as a consequence of this they break up, and thereby give rise to the ferment and paraglobulin.

The fibrinogen is pre-existent in the plasma.

According to this view, then, the essential element in coagulation is the death of the white blood corpuscles.

The results of my experiments are totally opposed to this view.

They are shortly stated as follows:—

The injection of a large quantity of dead lymph cells into the blood of a living dog, both in its normal state and when it has been injected with peptone, has no marked influence on the functions of the animal. No sign whatever of emboli can be detected after the animal is dead.

I give one example:—

A dog was peptonised; five minutes later a small quantity of blood was removed and divided into two portions, to the one lymph cells were added, it coagulated immediately, exactly like normal coagulation. The other portion remained uncoagulated for hours.

After the removal of this small test quantity of blood, a very large quantity of dead lymph cells, suspended in ½ per cent. solution of common salt, were injected into the animal. The latter was apparently quite unaffected by this. It was in the narcotic state always produced by peptone, but it was able, in the course of three-quarters of an hour, to walk about. No coagula were found on post-mortem examination.

These facts are of great importance, for they show that coagulation is the result of a change in the plasma, and has nothing to do with the vital properties of the cells; and they further fully confirm what I have endeavoured clearly to bring out, that the conversion of the white cells into fibrin is quite independent of the presence of any "fibrinogen" substance. Fibrinogen is present in "living" plasma, yet the dead cells produce no coagulation. Fibrinogen was absent from the peptone plasma, which still gave practically unlimited quantities of fibrin with lymph cells.

Now, Alexander Schmidt has shown most distinctly that white blood cells do unquestionably disappear as such during the normal coagulation of the blood; and, in another communication I shall confirm this in a most decided manner. I feel, therefore, justified, although I have, at present, only fully worked out peptone plasma, in saying that there are two essential processes in the coagulation of the blood, one of which has been hitherto entirely wrongly appreciated or overlooked. This latter process is that the "dead" plasma converts the white corpuscles directly into fibrin. At the same time, however, that this occurs, a substance is liberated from the cells which converts the fibrinogen also into fibrin. This is the other process. The substance which is liberated from the cells is fibrin ferment.

XVII. "A New Line of Research bearing on the Physiology of Sugar in the Animal System." By F. W. PAVY, M.D., F.R.S. Received June 8, 1881.

Twenty-three years ago I presented a communication to the Royal Society, entitled "On the alleged Sugar-forming Function of the Liver."

Four years previously, viz., in 1854, whilst conducting experiments directed towards determining the manner in which the sugar presumed, under the glycogenic doctrine, to escape from the liver was destroyed (as it was then believed to be) in the lungs, I discovered that what had been taken as representing the natural condition of the liver, and of the blood escaping from it in relation to sugar, was founded upon a fallacious inference. By those who have only been acquainted with what, in recent times, has been recognised as constituting the state existing, the original position in which the matter stood will hardly be fully comprehended. The strongly saccharine state in which the liver and the blood of the hepatic veins are found shortly after death was looked upon, without any question being raised about it, as representing the state existing during life. Without the slightest prior conception that such was likely to be the case, I found first that the blood between the liver and the lungs was not during life in the condition that had been supposed, and next that what I discovered for the blood applied also to the liver. The evidence which presented itself led me, as is known, to dispute the validity of the glycogenic theory, and the additional information which I have since from time to time obtained has materially strengthened the position I took. To my own mind, the conditions that we have to deal with looked at in their entirety, are totally irreconcilable with the glycogenic theory; but I know that the difficulty which has existed in accounting for the disposal of the glycogenic matter of Bernard encountered in the liver has stood in the way of a general adoption of my views. This subject, however, I am now prepared to approach and consider.

When the glycogenic matter was discovered, it was described as undergoing transformation into sugar immediately it was brought into contact with blood. Bernard said of it "Cette dernière [matière glycogène du foie] est tellement alterable qu'elle ne peut pas exister dans le sang sans être immediatement changée en sucre, de sorte qu'elle ne peut jamais sortir du foie que sous cet état." This impression has governed the position left open to us to take in relation to the disposal of the material, and rendered it necessary to look for some undiscovered mode of transformation of it within the liver under the presumption that it does not reach the circulatory system as sugar.

We have here a fundamental point to deal with; and, as in my original communication to the Society, I had to commence by clearing the ground of error, so now it happens that I have to proceed in a similar way in relation to the point in question. I have results to communicate which appear to me to place us in a new position, but before these are considered it is requisite that the knowledge upon which we start should be set right.

Whatever may be the future of the glycogenic theory, the substance which Bernard was the first to recognise will stand as it exists now. The name "Glycogen" was applied to it when certainly an erroneous notion existed regarding the condition of the liver and of the blood in relation to sugar. Its presumed destination suggested the application of the term. I have hitherto objected to its employment, and I have stronger grounds for doing so now. To avoid the incorporation of theory, I have spoken, in my previous writings, of the body in question under the provisional name of "amyloid substance." Something more definite than this is for final purposes required, and it appears to me that it would be a fitting tribute to the memory of its discoverer to call it "Bernardin." Such a name will be at once suggestive of the substance to which it is intended to refer, and will form an imperishable memorial, which, whatever doctrine may prevail, will serve to identify the person, to whom all must admit physiological science owes so much, with the subject which formed the most prominent field of his labours.

Entering now upon the results which it is the object of this communication to make known, I will begin with the experiments bearing on the effect of bringing Bernardin (glycogen) into contact with blood.

In these experiments the Bernardin was dissolved in a small quantity of water, and mixed with the blood in a defibrinated state. Sheep's blood was the kind of blood used. The product was placed in an oven specially constructed for such a purpose, and maintained for half an hour at a temperature between 100° and 110° F. It was then subjected to examination, and the quantity of sugar present ascertained by the ammoniated cupric test.*

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* The ammoniated cupric test referred to was described by me in former communications published in "Proc. Roy. Soc." vol. 28, p. 260, and vol. 29,

In some instances, the process of preparation for titration with the test consisted of extraction with alcohol, and in others of precipitation of the albuminous and colouring matters by heat with the aid of sulphate of soda. Bernardin (glycogen) prepared in different ways was used, and no difference in the results was perceived. Some of the specimens employed were obtained by simple extraction from the liver with water and precipitation with spirit. The actual condition of the specimen used as regards purity was ascertained by subjection to the converting action of sulphuric acid and heat, and the estimation of the glucose formed. In this way, the information was obtained for supplying the figures found in the subjoined table representing the Bernardin used, expressed in its equivalent of glucose. The figures representing the sugar produced were obtained by deducting from the sugar found in the product of experiment the figures yielded by a specimen of the blood alone purposely exposed to parallel conditions.

Bernardin (glycogen) added to Blood and exposed for half an hour to a temperature of 100° to 110° F.

	Amount of blood used.	Amount of sugar producible from the Bernardin mixed with the blood.	Sugar produced.
	cub. centims.	grm.	grm.
Experiment 1........	60	0·267	0·007
" 2........	60	0·267	0·018
" 3........	30	0·535	0·005
" 4........	30	0·535	0·005
" 5........	25	0·232	0·009
" 6........	50	0·432	0·014
" 7........	50	0·288	0·021
" 8........	50	0·144	0·007
" 9........	50	0·294	0·009
" 10........	50	0·147	0·009
" 11.......	50	0·144	0·036
" 12........	50	0·144	0·033
" 13........	25	0·144	0·007
" 14........	50	0·144	0·009
" 15........	25	0·144	0·000
" 16........	25	0·112	0·003
" 17........	25	0·112	0·002

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p. 272. Since bringing it under the notice of the Society, I have had a very large experience with it, and am thus enabled to speak in definite and confident terms about it. Its facility of application, delicacy, and precision place us in a most advantageous position in relation to the quantitative determination of sugar. An important feature also belonging to it is that its action is not interfered with by the presence of nitrogenous matter as is the case with the ordinary cupric solution. Under my supervision it has been used, I am quite within bounds in saying, some

The total of these results is that Bernardin (glycogen) equivalent to 4·085 grms. of glucose was added to 705 cub. centims. of blood, and that the amount of sugar produced was 0·194 grm. I do not know the reason of the figures standing so much higher in experiments 11 and 12 than in any of the others. The two were conducted together and with the same specimen of blood, which was only used in these particular experiments.

The amount of sugar produced is too insignificant to be susceptible of recognition by ordinary testing. Half a gramme of a sample of Bernardin (glycogen), standing equivalent to 0·367 grm. of glucose, was mixed with 25 cub. centims. of blood, and exposed for half an hour to 100° F. Prepared for testing with heat and sodic sulphate in the ordinary way, the product gave with the cupro-potassic test no visible indication of the production of sugar.

The point shown by these experiments is that Bernardin (glycogen) may be brought into contact with blood and kept in contact, for some time, at a temperature equal to that of the living body, without undergoing conversion into sugar to more than what may be spoken of as quite an insignificant extent. Not only do the quantitative determinations of sugar prove this, but there is the further conclusive fact that the Bernardin is subsequently recoverable from the blood. The ground is therefore cleared in such a manner as to show that there is nothing inconsistent with Bernardin existing in and constituting a natural element of the blood.

Having established this foundation, I will now proceed to deal with the blood as it exists in the body, investigated in relation to Bernardin.

It is only when light begins to dawn upon a subject that the path of

thousands of times; and if an experience of this kind will justify an expression of opinion, I may state that I am satisfied it will be found invaluable alike to the physiologist, the chemist, and the physician.

The test is prepared by adding ammonia to a cupro-potassic solution, for which the following is the formula:—

Cupric sulphate	34·65 grms.
Potassic sodic tartrate (Rochelle salt)	170 ,,
Potash	170 ,,
Water	to 1 litre.

For the ammoniated test 120 cub. centims. of the above solution are mixed with 300 centims. of strong ammonia (sp. gr. 0·880), and water added to a litre. 20 cub. centims. of this ammoniated cupric liquid are decolorised by 0·010 grm. of glucose.

The liquid to be examined is allowed to drop from a burette into a suspended flask containing the test, which is kept briskly boiling till decoloration has been attained. The reduction being attended with decoloration without precipitation, there is nothing to obscure the determination of the moment when the precise point wanted has been reached.

investigation, which is calculated to lead to an extension of knowledge, appears in view. Until a definite purpose to work up to, suggested by something or other which has been extricated from the realm of darkness is attained, engagement in the laboratory operations of research is unsatisfactory employment. For some time, I persevered in conducting general analytical examinations of the blood and liver, hoping that, through the results obtained, light might appear in some direction or other, but they failed to lead to any useful acquirement of knowledge. Later on, from information supplied by a collateral course of inquiry, I was induced to conduct examinations by another method of procedure, which was not likely to have been hit upon accidentally; and through these a new field has been opened out, which, if I judge rightly, has given signs of proving productive of an important addition to our knowledge.

It would scarcely occur to a person, unless forced upon him by what he had otherwise observed, to think it necessary in relation to the matter under consideration, to preserve the coagulated residue of blood and submit it to examination. It will be seen, however, from what has to be stated, that besides a certain amount of glucose, which may be removed by alcohol, blood contains a principle which agrees with Bernardin (glycogen) in being insoluble in alcohol, and convertible into glucose by exposure to the influence of sulphuric acid and heat. Some of this principle is dissolved out by water under aqueous extraction, but the remainder clings to the coagulated residue, which has to be subjected to special treatment in order that it may be brought into view. Thus, unless a special mode of examination is adopted which is not likely to suggest itself accidentally to the mind, the principle in question incorporated with the coagulated residue will remain concealed from observation.

In conducting a full or detailed examination of blood, the first step to be undertaken is the separation of the glucose which it contains for quantitative examination. Let 25 to 50 cub. centims. of defibrinated blood be poured into five or six times their volume of spirit. The glucose being soluble in alcohol is susceptible of extraction by this liquid. It is held, however, more tenaciously by the coagulated matter than might be expected, and hence to effect a complete removal several washings and pressings are required. At first I was deceived through not fully realising this fact, and thought I had obtained evidence of the presence of glucose, of a maltose-like body giving a greater cupric oxide reducing action after being subjected to the converting influence of sulphuric acid and heat than before, and of a substance agreeing with Bernardin (glycogen) in blood. The first I found in the alcoholic extract, the second in the aqueous extract, and the third in the solid residue. The maltose-like body, I have since ascertained, has no real existence. The cupric oxide reducing action in the aqueous extract noticeable before subjection to sulphuric acid and heat, arose from the glucose not having been thoroughly extracted in the alcoholic process, and the increased reducing action after sulphuric acid and heat was due to the presence, in addition to the glucose referred to, of a little Bernardin (glycogen) dissolved out from the residue by the water.

The plan that I adopt for effecting the complete removal of the glucose is as follows:—The defibrinated blood which has been taken for examination is poured into the requisite quantity of spirit, and the two are well stirred together. I am under the impression that it is advantageous for the coagulum to be allowed to remain in contact with the spirit till the following day. After being boiled by the heat of the water bath, the alcohol is strained off through a piece of linen material which has been cleansed so as to be free from dressing. Washing with alcohol is performed, and the coagulum in the linen is subjected to forcible squeezing in a suitable sized press. The residue, which by this process is converted into a dry cake, is pulverised in a mortar, mixed with fresh spirit, boiled over the water-bath, and again strained and pressed. The process is repeated once more, and this I find is sufficient. Thus extracted three times, there is practically no glucose left in the solid residue, and when an aqueous extract is made it gives no appreciable cupric oxide reducing action before subjection to the influence of sulphuric acid and heat.

Two extractions with alcohol might prove sufficient if carefully made, but it is safer to use three. To give the representations of actual results yielded, 50 cub. centims. of sheep's blood were extracted with alcohol. The first alcoholic extract contained 15 mgrms. of glucose, the second 4, and the third no definite amount. Some sugar from diabetic urine was added to sheep's blood, and 50 cub. centims. taken. The first alcoholic extract was found to contain 144 mgrms. of glucose, the second 9, and the third nothing definite.

To prepare the alcoholic extraction for testing, the mixture of alcoholic liquids obtained is acidified with acetic acid, heated to near boiling point over the water-bath, and then filtered through ordinary filtering paper. It is now brought down by heat to a small bulk, and treated with an excess of crystals of sulphate of soda with the view of causing the fatty matter finely dispersed through the liquid to agglomerate so as to be susceptible of removal by filtration. Water is added to the surplus crystals of sulphate of soda, and a hot solution made which is used for washing purposes.

The titration of the product of alcoholic extraction with the ammoniated cupric test gives quantitative results which stand in complete accord with those I obtained by the gravimetric process, which were mentioned in communications published in "Proc. Roy. Soc.," vol. 26, pp. 314 and 346.

For the purpose of ascertaining what kind of sugar is contained in the alcoholic extract, the product of extraction has been titrated with the ammoniated cupric test *before* and *after* subjection to the converting influence of sulphuric acid and heat. The resutts have shown that no increased reducing action is given after treatment with sulphuric acid and heat.

The cupric oxide reducing principle, therefore, which is extracted by alcohol from blood, consists of glucose.

When the alcoholic extraction has been thoroughly effected, the coagulated residue of blood fails, as I have stated, to yield to water anything possessing *per se* a cupric oxide reducing property. After treatment, however, with sulphuric acid and heat the aqueous extract is found to exert a certain amount of cupric oxide reducing action. The substance removed by water which possesses this property is part of the material present in the coagulated residue, which closely resembles, if it does not actually constitute, Bernardin (glycogen). There appears to be uncertainty in the amount extracted by water, and I think it may be considered that no useful information is derivable from the examination of the aqueous extract. Just for the purpose, however, of giving a representation of what I have found, I may state that taking the several observations of which I have a record (thirty-two in number), the amount of material contained in the aqueous extract convertible into cupric oxide reducing substance by sulphuric acid and heat, stands at about 0·290 per 1,000 expressed as glucose.

It did not occur to me, at starting, to do anything in the way of examining the residue left after alcoholic and aqueous extraction, not imagining that there would be anything of concern to me present. In the course of investigation, however, grounds presented themselves for leading me to deal with the residue, and I determined to treat it as I have been in the habit of treating the liver in making a quantitative determination of Bernardin (glycogen).

This process consists of dissolving by means of an alkali, pouring into spirit, and collecting the precipitate. Bernardin (glycogen) possesses two properties which greatly facilitate its separation from other bodies, viz., resistance to the action of an alkali and insolubility in alcohol. Albuminoid matters are attacked by caustic alkalies, and are then not precipitable by spirit as before. A method of separation is thus supplied. The object of applying the process was to ascertain whether there existed concealed in the residue anything of the nature of Bernardin (glycogen).

The residue was treated with water, and caustic potash added in the proportion of about one-fifth of the original weight of blood taken for examination. Heat was applied until solution occurred, and the product was poured into about five or six times its volume of spirit. The precipitate was allowed till the following day to settle, and the spirit was then carefully decanted off and some fresh spirit added and time again given to settle. The next step was to purify the product, and this was done by collecting and dissolving it in a little water, thoroughly acidifying the solution with acetic acid, filtering from the precipitate produced, and then re-precipitating with spirit. The precipitate thus finally obtained was dissolved in water and examined. The solution was found to exert no reducing action upon the ammoniated cupric test before treatment with sulphuric acid and heat, but did so afterwards. Such is the behaviour of Bernardin (glycogen), and such behaviour, it is to be noted, was obtained from a product furnished by the process of preparation adapted for yielding in a separated form whatever of the principle in question the blood might contain.

I have spoken of the material I have been referring to, which is precipitable by spirit and convertible into glucose by sulphuric acid and heat, under the name of Bernardin, but I am not prepared to state that the principle which exists in the blood is absolutely identical with that belonging to the liver. The determination of this point must form the subject of future investigation.

The process I have described is the one by which I learnt that a principle agreeing with Bernardin (glycogen) may be recognised in the blood. A shorter method, however, may be had recourse to for revealing its presence. The blood may be at once treated with potash and the product poured into spirit. The subsequent steps to be followed are those which have been already described.

It is through the ammoniated cupric test that I have been led on to the acquirement of the information that I have obtained. The delicacy of the test, and the circumstance that its action is not interfered with by the presence of a small quantity of nitrogenous matter, have enabled me to discern conditions which the ordinary application of the cupro-potassic test would have failed to have revealed. Once in possession of the knowledge supplied through the ammoniated cupric test, I could see that the ordinary cupro-potassic test ought to be susceptible of being rendered applicable for revealing the glucose produced from the Bernardin (glycogen) existing in blood, and such, I find, proves to be the case. From half a litre to a litre of blood should be taken and treated by being at once boiled with potash and poured into spirit. The aqueous solution of the ultimate alcoholic precipitate obtained possesses, as will be understood, not the slightest cupric oxide reducing power of its own; but, after treatment with sulphuric acid and heat, and subsequent neutralisation, gives a good reaction with the cupro-potassic solution used in the ordinary way. The suitability of blood for operating upon to show this result varies, and a specimen should be procured from an animal in good condition and well fed up to the time of death. The blood obtained from the slaughter-house is not always a favourable specimen for the purpose, and this is scarcely to be wondered at, looking at the abnormal state the animals often exist under for a day or two previous to being killed. Similarly, it is found that for collecting Bernardin (glycogen) from the liver, the slaughter-house often affords a very unfavourable specimen for the purpose.

I will furnish the results of the quantitative analyses I have performed. They were simply taken just as the specimens happened to present themselves. As yet I have not done anything towards ascertaining in a systematic way how the quantity of the principle is modified by antecedent conditions. This will form a subject for subsequent investigation. The glucose figures yielded by the analysis have been brought into Bernardin (glycogen) figures by the method of calculation which will be later on referred to.

Amount of Bernardin (glycogen) found per 1,000 of Blood.

Sheep.		Bullock.	Cat.	Rabbit.	Horse.
0·621	0·388	0·540	0·837	0·858	0·504
0·422	1·025	0·522	0·423		
0·540	0·605		0·387		
0·630	0·709		0·441		
0·549	0·594		0·630		
0·432	0·687		0·967		
0·623	1·483		0·693		
0·423	0·261				
0·797	0·283				

The mean of these twenty-nine results, taken altogether, gives 0·616 as the amount of Bernardin found per 1,000 of blood.

It has been seen that the method I adopt for the quantitative determination of the Bernardin present in the blood, is the conversion of it into glucose by the agency of sulphuric acid and heat, and then estimating the glucose by means of the ammoniated cupric test. This is the plan adopted for the quantitative determination of starch. I have experimented to ascertain the conditions required for securing complete conversion into glucose, and have found that Bernardin offers a far greater resistance to the converting action of sulphuric acid and heat than I had at first anticipated. The particulars of an observation are before me which showed that, after the process of boiling had been carried on for four hours, full conversion had taken place; whilst, at the end of three hours, conversion was considerably short of complete.

Fortunately for the progress of investigation, it happens that by exposure to a higher temperature under pressure, a comparatively short time suffices for effecting the complete conversion of Bernardin into sugar. If the necessity existed for having recourse to such a prolonged operation as boiling for four hours, a serious obstacle would be offered to the performance of the quantitative determination to which we have to look for the supply of information. To shorten the time of exposure, I at first employed a stout glass flask with a vulcanised rubber bung wired into its mouth, and immersed this in an oil-bath heated to a considerably higher temperature than that of boiling water. Finding that I could not make sufficiently rapid progress with my investigations by proceeding in this way, I had a stout copper boiler constructed for the application of heat under pressure. It is provided with a lid which is screwed down with iron bolts, and has a lever safety-valve with a shifting weight to regulate the pressure. The boiler is large enough to receive a number of flasks, so that several products can be operated on at a time. I am in the habit of working it at 45 lbs. pressure, which gives a temperature just under 300° F. At this temperature, the conversion into glucose is accomplished within about a quarter of an hour. It is reliably secured by exposure for half an hour, and this time I am in the habit of allowing.

The quantity of sulphuric acid used is in the proportion of about 4 per cent. of the strong acid to the volume of liquid that is being worked upon. It is essential to give attention that the conditions are not such that the acid is appropriated to the liberation of another acid unendowed with the power of exerting a converting influence: such, for instance, as would occur if acetic acid had been previously used so as to leave a considerable quantity of acetates present.

From the amount of glucose, which, through the ammoniated cupric test has been ascertained to be produced, the quantity of Bernardin (glycogen) is calculated. The accepted formula for Bernardin is the same as for starch, viz., $C_6H_{10}O_5$. The formula for glucose is $C_6H_{12}O_6$. The equivalent of the one is 162 and of the other 180. From these we obtain the factor for the conversion of glucose figures into Bernardin figures:—

Let x = the amount of Bernardin equivalent to one part of glucose.

Then, as $$180 : 162 :: 1 : x,$$

or $$x=\frac{162}{180}=\frac{9}{10}=\cdot 9.$$

Hence ·9 stands as the factor to multiply by to convert glucose figures into Bernardin figures.

I have shown that after the extraction of glucose from blood by alcohol, there is to be found a material which gives no cupric oxide reducing action before treatment with sulphuric acid and heat but does so afterwards. Water dissolves out a certain portion of this material; but the larger portion remains incorporated with the solid residue and is susceptible of being brought into view by boiling with potash and subsequent precipitation with spirit.

Bernardin (glycogen) added to blood behaves in a similar way. It is carried down, in great part, by the precipitated matter when coagulation is induced, and afterwards so tenaciously held that only the minor portion is removed by washing with water. The proportion dissolved out by water has varied considerably in different instances, and I have been led to question whether, in some cases, it may not be held to a more fixed extent than in others. I was at first much puzzled to know what had become of the substance, and it seemed, until I recognised it in the coagulated residue, that contact with blood led to an immediate extensive disappearance. To recover it, the coagulated matter requires to be subjected to disintegration by boiling with a caustic alkali. In some experiments, a notable amount of Bernardin has still remained unaccounted for, but, in some others, it has been nearly all recovered. For instance, in two recent experiments, after half an hour's exposure to a temperature of from 100° to 110° F., the following results were obtained: Bernardin equivalent to 0·112 grm. of glucose was in each case added, and Bernardin equivalent to 0·105 grm. of glucose in each case recovered. In another experiment Bernardin equivalent to 0·294 grm. of glucose was added, and Bernardin equivalent to 0·269 grm. of glucose recovered. I consider that further observations upon this subject require to be undertaken.

It is known that precipitates have a tendency to carry down other substances with them, and this principle, it may be suggested, accounts for what has been adverted to above. I am inclined to think, however, from a review of all the evidence before me, that there is something more than this at the foundation of what occurs. The question may be raised whether there is not some feeble combination existing, and whether this may not have a bearing of considerable physiological importance. Subjoined are results touching the point under consideration.

A weighed quantity of liver from a recently killed dog was reduced to a pulp and thoroughly extracted with alcohol for the removal of glucose. The coagulated residue from the alcoholic washing was then extracted with repeated washings of boiling water till the washings came away clear instead of lactescent as at first. The washings were collected and the Bernardin (glycogen) estimated by conversion into glucose and calculation from the glucose found. After being thus washed the liver residue was kept in a moist condition till the following day, when the process of washing and estimation of the extracted Bernardin was again performed. The process was subsequently repeated in a similar way; and, finally, the liver residue which had been subjected to the several daily washings was boiled with potash and the liquefied product poured into spirit and the Bernardin collected and estimated. The following are the figures that were yielded :—

	Per 1,000 of liver.
Bernardin extracted the 1st day	13·833
,, ,, 2nd ,,	3·879
,, ,, 3rd ,,	2·961
,, ,, 4th ,,	2·817
Bernardin remaining in the liver residue	35·145

In another similarly conducted experiment upon the liver of a cat the following results were obtained :—

	Per 1,000 of liver.
Bernardin extracted the 1st day	3·951
,, ,, 2nd ,,	1·134
,, ,, 3rd ,,	Trace
Bernardin remaining in the liver residue	3·609

The above liver, it will be seen, was poor in Bernardin; and this is often noticed in the cat, unless the animal has been fed with milk or bread in addition to, or in lieu of, meat.

In a third experiment upon the liver of a rabbit the following figures were furnished :—

	Per 1,000 of liver.
Bernardin extracted the 1st day	13·503
,, ,, 2nd ,,	3·376
,, ,, 4th ,,	3·688
,, ,, 8th ,,	2·466
,, ,, 12th ,,	2·299
Bernardin remaining in the liver residue	73·167

These results show how imperfectly ordinary extraction with water removes Bernardin from the liver substance, and account for the small quantities which, perhaps, may have been noticed to have been obtained when aqueous extraction has been had recourse to for collecting it.

Before the above quantitative experiments were performed, I noticed the following circumstance, which, indeed, led me to undertake them. Some bruised liver substance was washed in a beaker with boiling water, till the water failed to present the slightest appearance of lactescence. The last washing was poured off, and the liver substance simply left in a moist condition in the beaker, till the following day. On then boiling it up with water, a highly lactescent liquid (from the extraction of Bernardin) was again obtained. A few repetitions of the washing resulted, as on the previous day, in clear liquid being yielded, and the residue placed aside as before in a moist state, gave rise on the following day to a recurrence of the phenomena. In some way or other, the Bernardin is held in the tissue, and with the lapse of time is set free for being taken up by water.

Although what I have described is noticeable when the liver substance is extracted with water at 212° F., yet a very different result is attainable by extraction under pressure, at a temperature of 300° F. At this temperature, it is found that all the Bernardin is speedily extracted. The copper boiler or digester, which I employ for the conversion of Bernardin into glucose by sulphuric acid, has been made use of for the extraction of liver experiments at 300° F., and half an hour, as the following statement of results obtained shows, has sufficed for the full accomplishment of extraction.

Two weighed portions of rabbit's liver, which had been bruised to a pulp in a mortar, were treated with spirit, and washed and pressed for the removal of glucose. The solid residue was then, in the one case, boiled with a solution of potash, and the Bernardin precipitated with spirit, and afterwards estimated in the usual way by conversion into glucose. In the other case, the liver residue, after alcoholic extraction, was treated with water and extracted at 300° F. The aqueous extract was then boiled with a little potash, and the Bernardin precipitated with spirit and estimated by conversion into glucose. The figures yielded stood thus:—

	Bernardin per 1,000 of liver.
After solution of liver substance in potash........	91 ·314
After extraction at 300° F.	93 ·015

In a second similarly conducted experiment upon a rabbit's liver, the figures obtained were:—

	Bernardin per 1,000 of liver.
After a solution of liver substance in potash......	64 ·242
After extraction at 300° F.	68 ·040

In a third experiment upon a cat's liver, the results were:—

	Bernardin per 1,000 of liver.
After solution of liver substance in potash........	30 ·735
After extraction at 300° F.	31 ·742

Whatever the precise explanation, it follows from the results which have been set forth that the manner in which Bernardin (glycogen) is held, or the condition under which it exists in the liver, is such that at a temperature of 300° F., it is readily and completely susceptible of removal by water, whilst the resistance offered to removal is at the same time sufficient to permit only partial extraction to occur within moderate limits of time at the ordinary boiling temperature of water.

When referring to the Bernardin (glycogen) discoverable in the blood, I stated that I was not prepared to assert that the principle existing in the blood is absolutely identical with that belonging to the liver. There is a point of difference which I have noticed in relation to the effect of extraction with water at 300° F.; but whether this is due to a difference in the properties of the principle itself, or whether to the manner in which it is held by other matter, I am not yet in a position to decide. The Bernardin of the liver, as I have shown, is readily extracted with water at 300° F., whilst according to the experiments I have conducted, that of the blood is not similarly susceptible of removal.

I have not yet said anything about the structures of the body generally in relation to Bernardin. I have subjected the spleen, pancreas, kidney, brain, and muscle to the same kind of examination as I have adopted in the case of the blood, and have found in all a notable amount of Bernardin.

Muscle has long been known to contain Bernardin (glycogen), but the quantity I obtain by the potash process is decidedly greater than what I believe is generally thought to be present. From cat's muscle, in one of my observations, the analysis yielded upwards of 5 per 1,000. Horse's muscle has been known to be specially characterised as containing Bernardin, and 9 per 1,000 in one instance is the quantity I have obtained.

In the observations I have yet made upon the spleen, pancreas, kidney, and brain, the largest amount of Bernardin has been found in the spleen, and in one instance the quantity indicated was a little under 4 per 1,000.

Bernardin therefore has been found as a constituent of all the tissues I have up to the present examined. I have likewise obtained it in notable amount from both the white and yolk of egg.

Besides Bernardin, there is, in the several tissues I have referred to, a cupric oxide reducing substance, which is susceptible of extraction by alcohol, and this also is present to a somewhat notable extent. In the case of the spleen, pancreas, kidney, and brain, this body appears to be glucose, the reducing power in most of the instances having come out about the same before and after the treatment with sulphuric acid and heat. In the case of muscle, however, the reducing power has been usually observed to be about twice as great after the treatment with sulphuric acid and heat as before. It seems, therefore, that we have here a body of the nature of maltose, instead of glucose, to deal with.

Although I have a considerable number of observations before me, yet I consider it advisable, at present, only to speak in the general way that I have done upon this last subject. Observations conducted under varied physiological conditions require to be undertaken, so that besides having the facts as mere chemical facts before us, we may be in a position also to deal with them from a physiological point of view. In a subsequent communication, I will enter further into this matter, and then supply details of the actual quantitative results.

Summary of Conclusions.

Bernardin (glycogen) does not undergo any significant transformation into sugar in contact with blood.

Bernardin exists to a distinctly notable extent as a normal constituent of blood.

The evidence derivable from the observations recorded on the addition of Bernardin to blood and its subsequent recovery, and on its extraction from the liver by boiling water on successive days, and by water at 300° F., tends to show that Bernardin enters into feeble combination with nitrogenous matter.

Bernardin exists in notable amount, not only in muscle, as has been previously known, but also in the spleen, pancreas, kidney, and brain. These are all the structures I have yet examined. It also exists in notable amount in the white and yolk of egg. These several products likewise contain a cupric oxide reducing substance, which is extracted by alcohol, and which, in most instances, possesses the characters of glucose, but, specially in the case of muscle, the characters of maltose.

Through the existence of Bernardin (glycogen) throughout the system, as has been represented, we have a carbohydrate occupying a parallel position to albumen, viz., existing in the colloidal state, and thus adapted for retention within the body, instead of passing off as a diffusible substance as glucose tends to do.

XVIII. "On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains." By G. H. DARWIN, F.R.S. Received June 11, 1881.

(Abstract.)

In this paper I have considered the subject of the solidity and strength of the materials of which the earth is formed from a point of view from which it does not seem to have been hitherto discussed.

The first part of the paper is entirely devoted to a mathematical investigation, based upon Sir William Thomson's well-known paper on the rigidity of the earth.* The second part consists of a summary and discussion of the preceding work.

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* "Thomson and Tait's Nat. Phil.," § 834, or "Phil. Trans.," 1863, p. 573.

The existence of dry land proves that the earth's surface is not a figure of equilibrium appropriate for the diurnal rotation. Hence the interior of the earth must be in a state of stress, and as the land does not sink in, nor the sea-bed rise up, the materials of which the earth is made must be strong enough to bear this stress.

We are thus led to inquire how the stresses are distributed in the earth's mass, and what are magnitudes of the stresses.

In this paper I have solved a problem of the kind indicated for the case of a homogeneous incompressible elastic sphere, and have applied the results to the case of the earth.

If the earth be formed of a crust with a semi-fluid interior, the stresses in that crust must be greater than if the whole mass be solid, very far greater if the crust be thin. As regards the condition of incompressibility attributed to the materials of the earth, it is proved in this paper that the compressibility of the solid would make no sensible difference in the results; except, indeed, in the case where the deformation of the sphere is of the second spherical harmonic class, when large compressibility would considerably modify the results.

The strength of an elastic solid is here estimated by the difference between the greatest and least principal stresses, when it is on the point of breaking, or, according to the phraseology adopted, by the breaking stress-difference. The most familiar examples of breaking stress-difference are when a wire or rod is stretched or crushed until it breaks; then the breaking load divided by the area of the section of the wire or rod is the measure of the strength of the material. Stress-difference is thus to be measured by tons per square inch.

Tables of breaking stress-differences for various materials are given in the paper.

The problem is only solved for the class of inequalities called zonal harmonics; these consist of a number of waves running round the globe in parallels of latitude. The number of waves is determined by the order of the harmonic. In the application to the earth the equator here referred to may be any great circle, and is not necessarily the terrestrial equator. The second harmonic has only a single wave, and consists of an elevation at an equator and depression at the pole; this constitutes ellipticity of the spheroid. An harmonic of a high order may be described as a series of mountain chains, with intervening valleys, running round the globe in parallels of latitude, estimated with reference to the chosen equator.

The case of the second harmonic is considered in detail, and it is shown that the stress-difference rises to a maximum at the centre of the globe, and is constant all over the surface. The central stress-difference is eight times as great as the superficial.

On evaluating the stress-difference arising from given ellipticity in a rotating spheroid of the size and density of the earth, it appears that if the excess or defect of ellipticity above or below the equilibrium value were $\frac{1}{1000}$, then the stress-difference at the centre would be 8 tons per square inch; and that, if the sphere were made of material as strong as brass, it would be just on the point of rupture. Again, if the homogeneous earth, with ellipticity $\frac{1}{232}$, were to stop rotating, the central stress-difference would be 33 tons per square inch, and it would rupture if made of any material excepting the finest steel.

The stresses produced by harmonic inequalities of high orders are next considered. This is in effect the case of a series of parallel mountains and valleys, corrugating a mean level surface with an infinite series of parallel ridges and furrows.

It is found that the stress-difference depends only on the depth below the mean surface, and is independent of the position of the point considered with regard to ridge and furrow.

Numerical calculation shows that if we take a series of mountains, whose crests are 4,000 meters, or about 13,000 feet, above the intermediate valley bottoms, formed of rock of specific gravity 2·8, then the maximum stress-difference is 2·6 tons per square inch (about the tenacity of cast tin); also if the mountain chains are 314 miles apart, the maximum stress-difference is reached at 50 miles below the mean surface.

The solution shows that the stress-difference is *nil* at the surface. It is, however, only an approximate solution, for it will not give the stresses actually in the mountain masses, but it gives correct results at some three or four miles below the mean surface.

The cases of the harmonics of the 4th, 6th, 8th, 10th, and 12th orders are then considered; and it is shown that, if we suppose them to exist on a sphere of the mean density and dimensions of the earth, and that the height of the elevation at the equator is in each case 1,500 meters above the mean level of the sphere, then in each case the maximum stress-difference is about 4 tons per square inch. This maximum is reached in the case of the 4th harmonic at 1,150 miles, and for the 12th at 350 miles, from the earth's surface.

In the second part of the paper it is shown that the great terrestrial inequalities, such as Africa, the Atlantic Ocean, and America, are represented by an harmonic of the 4th order; and that, having regard to the mean density of the earth being about twice that of superficial rocks, the height of the elevation is to be taken as about 1,500 meters.

Four tons per square inch is the crushing stress-difference of average granite, and accordingly it is concluded that at 1,000 miles from the earth's surface the materials of the earth must be at least as strong as granite. A very closely analogous result is also found from the discussion of the case in which the continent has not the regular wavy character of the zonal harmonics, but consists of an equatorial elevation with the rest of the spheroid approximately spherical.

From this we may draw the conclusion, that either the materials of the earth have about the strength of granite at 1,000 miles from the surface, or they have a much greater strength nearer to the surface.

This investigation must be regarded as confirmatory of Sir William Thomson's view, that the earth is solid nearly throughout its whole mass. According to this view, the lava which issues from volcanoes arises from the melting of solid rock, existing at a very high temperature, at points where there is a diminution of pressure, or else from comparatively small vesicles of rock in a molten condition.

XIX. "On the Refraction of Electricity." By ALFRED TRIBE, F.I.C., Lecturer on Chemistry in Dulwich College. Communicated by Dr. GLADSTONE, F.R.S. Received June 7, 1881.

On December 15, 1880, I had the honour of communicating to the Royal Society the latest results of my work on electric distribution. In that paper there is included a description of results which form the basis of a graphic and electro-chemical method of investigating the field of electrolytic action. These results may be classed under three heads:—1st. Distribution of electricity on metallic conductors in electrolytic media. 2nd. Physical differences in corresponding parts of non-homogeneous electrolytic fields. 3rd. Direction in which the energy is transmitted.

As the detailed account of these experiments has not yet been published, it is necessary for the appreciation of the evidence to be adduced to give in this place the groundwork of that part of the method relating to the direction in which the energy is transmitted. It will be convenient to do this under three heads. Let it be remembered that a rectangular electrolytic cell was used, that the electrolyte was a solution of copper sulphate, and the electrical relations of the liquid were ascertained by immersing in it a rectangular silver plate (called an *analysing plate* or *analyser*), on which the ions were deposited. In all cases the positive ion separates and is distributed on that part of the plate which may be supposed to receive — electrification, or by which the + energy enters the analyser, while the — ion separates on that part of the same plate which receives + electrification, or from which the energy emerges.

a. When the course of the energy* is parallel with any two edges of an analyser, and therefore with the sides, the boundary lines of the ions on both sides of the plate are parallel with the plane of the elec-

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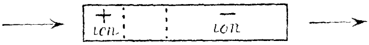
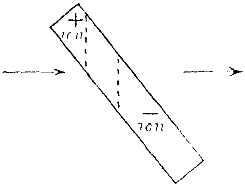
[*] I assume that the energy in a homogeneous field runs in straight parallel lines from one electrode to another, and that this course is not appreciably disturbed by an analyser.

trodes, and consequently perpendicular to the direction of the influence. Distributions having these characteristics are named *parallel.* The dotted lines in the figure represent the boundary lines and the arrows the course of the energy.



β. When the course of the energy is parallel with the sides of an analyser, but makes with its edges an oblique angle, the boundary lines of the ions are still parallel with the plane of the electrodes, but necessarily cross the sides of the plate obliquely to its edges. This obliquity varies proportionately with the angle of inclination of the analyser to the energy, so that in all cases the boundary lines are perpendicular to the direction of its transmission.



γ. When the course of the energy makes an oblique angle with the *sides* of the analyser, neither the magnitude nor the boundary configuration of the same electrification is identical on the two sides of the plate. The boundary of the positive ion on the side in opposition to the direct course of the influence is now markedly *convex*, and greater in magnitude than on the reverse side, where, moreover, the boundary configuration of the same ion is markedly concave. Again, the boundary line of the negative ion on the first-named side of the analyser is concave and smaller in magnitude than on the reverse side, where the boundary of this ion is convex. The convexity and concavity of these several boundaries increase as the direction of the energy approaches a perpendicular to the sides of the analyser. Both the classes of distributions described in β and γ are named non-parallel.

The position of the ions on an analyser, and the character of their boundary lines determine then, and with accuracy, the direction of the energy in the electrolytic field relatively to either side or edge of the analysing plate.

Many questions have occurred to me in the investigation of which this method might be expected to afford material assistance. But the one of immediate attraction was whether electricity is endowed, like light, heat, and sound, with the quality of refraction. From the general resemblance of the fundamental laws of the forms of energy, I instituted experiments in the expectation of finding an answer in the affirmative to this question, the better conducting electrolytic medium being taken as the electric analogue of the more rare medium in light.

Refraction.

My first trials were made with double convex-shaped bladders. These gave what I took to be a slight evidence of refraction, though the result was far from satisfactory. Triangular-shaped cells were next employed, made by placing diaphragms of parchment-paper obliquely across the electrolytic cell near its ends. In this way unmistakeable proof was obtained of the bending of the energy in passing the line of demarcation of the two media. When the influence passed from one medium to the other perpendicularly, *i.e.*, when the diaphragms were parallel to the plane of the electrodes, no refraction whatever took place.

On further consideration the arrangement which appeared less open to objection, and at the same time the most simple and theoretically the best, was a *refracting cell*, having parallel sides of some material permeable to the electric influence. In the first instance parchment-paper was the material employed. Two sheets of this substance were fixed in a vertical position across an electrolytic cell, 380 millims. long, 128 millims. broad, and 128 millims. deep, at an angle of ·45°. They were parallel to one another, 76 millims. apart, but equidistant from the respective ends of the cell.

An unit current was employed, and copper electrodes of the breadth and depth of the outer or transmitting cell. A 1 per cent. solution of copper sulphate was placed in the inner or refracting cell, and a concentrated solution of the same salt in the transmitting cell. On placing successive analysers* lengthwise in several parts of the *central line* joining the electrodes, parallel distributions were recorded by all the plates in the transmitting cell, but the one in the refracting cell recorded a non-parallel distribution of a most pronounced character, and it was evident from the degree of curvature of the ions that the course of the energy on passing into the medium of less conductivity, had bent out of its original course some 20°.

The difficulty of keeping the parchment diaphragms as rigid as was necessary for a more extended study of this phenomenon of refraction led to their substitution by others of unglazed earthenware. These consisted of the sides of a large rectangular porous cell ground to as uniform a thickness as possible. With this alteration, but with all the other above-mentioned conditions, the following experiments were made.

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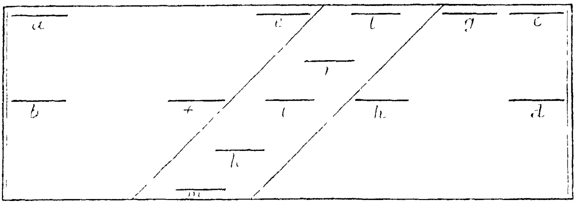
* Unless the contrary is stated, it is to be understood that analysers 40×7 millims. were used, and placed in the electrolyte with their shorter edges upright.

Analysers were successively placed in the positions (all perpendicular to the electrodes) shown in the annexed diagram, exhibiting a horizontal section drawn to about one-fifth the scale.



The analysers *a*, *b*, *c*, *d*, 2 millims. from electrodes, recorded parallel distributions.

The analysers *e*, *f*, *g*, *h*, 2 millims. from diaphragm, recorded not absolutely but very nearly parallel distributions. This slight non-parallelism was not noticed in the analogous experiment with the parchment diaphragms, and I am disposed to attribute it to a greater diffusion in this case occasioning a less sharp line of demarcation between the media.

The analyser *i*, midway between the diaphragms, recorded a non-parallel distribution of a most pronounced character. The degree of curvature showed that the energy had been refracted through some 30°, while the position of the ions proved that the bending was towards a perpendicular to the refracting surface. Furthermore, the symmetry of the curve showed that the plane of refraction was the same as that of the incident energy.

The analysers *j*, *k*, indicated a result almost identical with *i*.

The analysers *l*, *m*, showed a much smaller deviation from the original course of the energy.

The positions of the relatively good and bad conducting media were now reversed. The 1 per cent. solution was placed in the transmitting cell, and the concentrated in the refracting cell. An analyser at *i* recorded a non-parallel distribution, and the curvature and position of the ions showed that the course of the energy had been bent about 15°. But in this case the refraction was *from* a perpendicular to the refracting surface.

It was now natural to anticipate that the refraction, with a given incidence, would decrease or increase as the media in the refracting and transmitting cells approached or receded in conducting power. This was proved to be the case, first, by replacing the 1 per cent. solution of copper sulphate in the refracting cell by a 2, 3, 5, and 10 per cent. solution respectively, when analysers at *i* recorded respectively a decreasing refraction as the media in the cells approached equality. And, secondly, by placing a 5 per cent. solution of copper sulphate in the refracting cell, and successively a concentrated solution of copper sulphate, a concentrated solution of sodium chloride, and a dilute solution (1—11) of oil of vitriol in the transmitting cell. Analysers at *i* showed, in the case of the copper sulphate, a refraction of about 15°, in the case of the sodium chloride, 30°, and in that of the dilute sulphuric acid, 40°. The order of the increase in refraction is here the same as that of the conductivities of the liquids.

Again, it was anticipated that the refraction would decrease as the incidence decreased. This was proved to be true by setting the diaphragms so that the angle of incidence should be 45°, 30°, and 15° respectively. With a 5 per cent. solution of copper sulphate in the refracting cell, and dilute sulphuric acid in the transmitting cell, a refraction of about 35° was obtained when the incidence was 45°, about 22° when at 30°, and about 7° when at 15°.

Relation between Electric Incidence and Refraction.

That part of the electro-chemical method which has been already described supplies three means of determining the bending of the course of the energy on its passage into the refracting cell. First, by the curvature of the ions on an analyser with the shorter edges of the plate upright, and its length perpendicular to the electrodes. But the practical impossibility of obtaining accurate measurements of these curves, renders this plan only roughly quantitative. Secondly, by placing an analyser at various angles to a right line joining the electrodes, until it records a *parallel* distribution. This is the case when the course of the energy is parallel with the sides of the analysing plate, and the angle enclosed between the plate and the right line referred to consequently expresses the amount of refraction. The objections to this plan are twofold. It is very tedious, and it is also difficult to determine with sufficient accuracy the angle of the plate as it stands in the cell. The refraction numbers given above were obtained by this second method, and for the last of the reasons just mentioned I regard them only as approximately correct. The third plan, based on the fact that the course of the influence, when parallel to the sides of an analyser, is perpendicular to the boundary lines of the ions set free, is the one I have employed for the more accurate determination of the relation between electric incidence and refraction.

I employed for this purpose four similar electrolytic cells of the shape and dimensions given in p. 436. Pairs of porous plates were set across each of these cells, as before described, so as to give in one case an incident angle of 45° ; in the second, 37½° ; in the third, 30° ; and in the fourth, 15°. Dilute sulphuric acid (1 to 11) was used in the transmitting cell, and a 5 per cent. solution of copper sulphate in the refracting cell. The current generally used was three Webers, though variations in this respect were found not to affect the refraction. The time of each determination was five minutes. The analysers employed were squares (24 millims. the side) of sheet silver, and disks of the same material 24 millims. in diameter. These were immersed horizontally in the centre of the refracting cell, the squares in such a way that their edges were parallel with the respective sides and ends of the rectangular electrolytic cell; the disks, so that a line passing through their centres was coincident with a line joining the electrodes.

As it has been already shown that the course of the energy in the transmitting and refracting cells was in the same horizontal plane, it was to be anticipated that the boundary lines of the ions on the analysers placed in the positions just described would be straight. Such is the case with the boundary of the negative ion, which consequently is taken for purposes of measurement.

Were the course of the energy in the inner cell identical with that in the transmitting cell, the boundary lines, in accordance with the rule in β, would be perpendicular to a right line joining the electrodes, and this might be named the zero line, or line of no refraction. And were the analysers in the position described, the zero line would be perpendicular to the central line on the disks and to the edges of the squares, which are parallel with the sides of the cell. It follows that, were the course of the energy diverted from a right line joining the electrodes (its original course), the boundary line of the ion would deviate *pari passu* from the line of no refraction. The numbers expressing this obliquity or amount of refraction in five series of trials are given in the annexed table. In Series I, II, III, square analysers were used, and for IV and V the disks already described.

Angle of incidence.	Deviation.				
	I.	II.	III.	IV.	V.
15°	9°	9°	9°	10°	9°
30	19	20	19	19	20
37½	27	26	25	26	25
45	31	31	33	33	32

These data give the following ratios between the angles of incidence and refraction—

$$\frac{i}{r}.$$

I.	II.	III.	IV.	V.
2·5	2·5	2·5	3·0	2·5
2·7	3·0	2·7	2·7	3·0
3·5	3·3	3·0	3·3	3·0
3·2	3·2	3·7	3·7	3·4

and the following ratios between the sines of the angles of incidence and refraction—

$$\frac{\sin i}{\sin r}.$$

I.	II.	III.	IV.	V.
2·5	2·5	2·5	2·9	2·5
2·6	2·8	2·6	2·6	2·8
3·3	3·0	2·8	3·0	2·8
2·9	2·9	3·4	3·4	3·1

Taking the results as they thus appear, I think they justify the conclusion that the more probable relation between electrical incidence and refraction is as the sines of their respective angles. And further, as the experimental difficulties are overcome and the inherent sources of error appreciated, this relation may be still more rigidly found to obtain.

This electro-chemical method being new, it would be well to supplement, if possible, the evidence it has furnished of the laws of electric refraction by one based on more familiar principles. I am engaged in perfecting a method founded on those employed by De la Rive in 1825, and by Professor Adams in 1875, for investigating the laws of electric distribution in electrolytes, which I have reason to believe will demonstrate in another way the main facts set forth in this communication.

Conclusions.

I. Electricity passes without alteration of direction from one electrolytic medium to another differing from it in conductivity, when the course is perpendicular to the surfaces of contact.

II. Electricity, on passing obliquely from one medium to the other, suffers refraction, and in the same plane; towards the perpendicular, when from a better to a worse conductor, and from the perpendicular when from a worse to a better conductor.

III. The refraction increases or decreases as the media recede from or approach one another in conductivity.

IV. The refraction increases as the incidence increases.

[NOTE.—It is known that the flow of electricity in an electrolyte follows the same laws as the flow of heat in a conductor when the condition has become permanent. It readily follows, that when two electrolytes of different specific resistance, suppose a strong and a weaker solution of sulphate of copper, are in contact, a change in the direction of the flow takes place in passing out of the one into the other.

Let v be the potential in the first medium, i the angle of incidence, or in other words, the inclination of the tangent plane at any point in the surface of separation to the tangent plane to the equipotential surface in the first medium which passes through the same point, c the specific conducting power, ds an element of a section of the common surface by the plane of incidence, dn an element of a normal to the equipotential surface, f the flux of electricity, and let v', i', c', dn', f', be for the second medium what v, i, c, dn, f are for the first. Then we have

$$f=c\frac{dv}{dn}=c\operatorname{cosec} i\frac{dv}{ds}, \qquad f'=c'\operatorname{cosec} i'\frac{dv'}{ds}.$$

But if we take an elementary closed curve in the surface of separation, and make it the base of elementary tubes in the two media bounded by lines of flow, since the same quantity of electricity must flow through the two tubes, and the areas of their sections are as cos i to cos i', we have

$$f\cos i=f'\cos i',$$

and eliminating f/f' between these two equations, we get, since $v=v'$ and therefore $dv/ds=dv'/ds$,—

$$\tan i'=\frac{c'}{c}\tan i,$$

so that according to theory the tangents, not sines, of the angles of incidence and refraction are in a given ratio.

If the potentials in the two media, instead of being equal at the surface of separation, differ by a constant quantity, as may conceivably be the case when the two electrolytes are different in nature, so that different chemical actions go on in them, the above relation would not be disturbed.

The results obtained by Mr. Tribe agree rather better with the law of sines than with the law of tangents. But several circumstances tend to introduce greater or less errors. For example, the flow in the second medium must have been somewhat disturbed by the circumstance that the non-conducting walls of the intermediate cell were parallel to the direction of the incident, not refracted flow, which may possibly have disturbed the course even as far as the middle of the cell. Again, there would have been a much larger resistance in the porous wall than in a stratum of equal thickness of one of the electrolytes, and the thickness or porosity of the wall, or the proportion of the two electrolytes imbibed by it, may have varied somewhat in a lateral direction. The numbers obtained cannot therefore be deemed sufficient to decide between two such laws as that of sines and that of tangents. In case of the second medium being the better conductor, it is evident that the law of sines would lead to extravagant results, as there can be no such thing as total internal reflection. The alteration in the direction of the equipotential surfaces, and, therefore, of the lines of flow, in passing from one metal into another of different conducting power, has already been investigated experimentally by Quincke, and the results of experiment compared with theory. ("Pogg. Ann.," vol. xcvii [1856], p. 382.)—G. G. S., 14 June, 1881.]

XX. "Note on the Spectrum of Sodium." By Captain W. de W. Abney, R.E., F.R.S. Received June 14, 1881.

On examining the spectra of different metals, there is one point which is striking in the extreme, viz., the absence of any very marked lines in the region between λ 7000 and λ 7600, which latter number we may take as the visible limit of the spectrum. With the exception of the well-known pair of lines of potassium, I am not aware that any lines in metallic spectra, which have been carefully studied, have been found below this limit, though recently, in the spectra of some of the rarer earths, I believe some few lines have been recorded.

Having photographed the emission spectra produced in the arc of several metals, it appears, so far as examination has been made, that only those which can be volatilised at a low temperature have any lines in the infra-red region. Sodium is an example of this. It has a pair of lines at wave-length of about 8187 and 8199 of an intensity of about 3, taking the intensity of D lines as 10. It will be noted that the difference in wave-length between this pair is greater than that of the D lines. They do not seem to have any corresponding dark lines in the solar spectrum, though there are three faint lines which lie close to these wave-lengths.

In the calcium spectrum there is a pair of very faint lines which lie between λ 8500 and 8600. Their exact wave-lengths have not at present been determined.

Iron, cobalt, nickel, copper, magnesium, and potassium have, up to the present, given negative results, but will be examined again.

XXI. "Formulæ for sn $8u$, cn $8u$, dn $8u$, in terms of sn u." By ERNEST H. GLAISHER, B.A., Trinity College, Cambridge. Communicated by J. W. L. GLAISHER, M.A., F.R.S. Received June 16, 1881.

(Abstract.)

In Grunert's "Archiv der Mathematik und Physik," vol. xxxvi (1861), pp. 125–176, Baehr has given the formulæ for sn nu, cn nu, dn nu in terms of sn u for the cases $n=2, 3, 4, 5, 6, 7$. These expressions are reproduced in a tabular form in Cayley's "Treatise on Elliptic Functions," Art. 109.

The present paper contains the corresponding formulæ for the case of $n=8$. Denoting the numerators and common denominator of sn $4u$, cn $4u$, dn $4u$ by P, Q, R, S respectively, then the numerators and common denominator of sn $8u$, cn $8u$, dn $8u$ are respectively $2PQRS$, $S^4-2P^2S^2+k^2P^4$, $S^4-2k^2P^2S^2+k^2P^4$, $S^4-k^2P^4$; and the paper contains the values of these quantities and also of P^2, S^2, P^4, S^4 in terms of sn u, arranged in a tabular form.

XXII. "On Riccati's Equation and its Transformations, and on some Definite Integrals which satisfy them." By J. W. L. GLAISHER, M.A., F.R.S., Fellow of Trinity College, Cambridge. Received June 16, 1881.

(Abstract.)

The memoir relates chiefly to the different forms of the particular integrals of the differential equation

$$\frac{d^2u}{dx^2}-a^2u=\frac{p(p+1)}{x^2}u \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

and to the evaluation of certain definite integrals which are connected with this equation.

Transforming (1) by assuming $u=x^{-p}v$ and putting $2p=n-1$, it becomes

$$\frac{d^2v}{dx^2}-\frac{n-1}{x}\frac{dv}{dx}-a^2v=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

and this may be transformed into Riccati's equation,

$$\frac{d^2v}{dz^2}-a^2z^{2q-2}v=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

by the substitution $x=\frac{1}{q}z^q$ where $q=\frac{1}{n}$.

It is well known that these equations admit of integration in a finite form if $p=$ an integer, $n=$ an uneven integer, and $q=$ the reciprocal of an uneven integer.

The memoir consists of an introduction and eight sections, the headings of which are: (1) Direct integration of the differential equation in series and connexion between the particular integrals; (2) New integration of the differential equation when $p=$ an integer; (3) Transformations of the original differential equation; Riccati's equation; (4) Special forms of the particular integrals in the cases in which the differential equations admit of integration in a finite form; (5) Evaluation of definite integrals satisfying the differential equations; (6) Symbolic forms of the particular integrals in the cases in which the differential equations admit of integration in a finite form; (7) Connexion with Bessel's Functions; (8) Writings connected with the contents of the memoir.

In the first section six particular integrals of the equation (1) are obtained and the relations between them are examined. When p is not an integer all the six particular integrals extend to infinity and the relations between them present no special peculiarity. When p is an integer two of the series terminate, and we thus obtain two particular integrals of (1), which contain a finite number of terms. The series terminate in consequence of the occurrence of zero factors in the coefficients of the terms, but if they be continued, zero factors occur also in the denominators, so that, after a finite number of terms, the series may be regarded as recommencing and extending to infinity. If the terminating series are supposed to recommence in this manner, so that all the series extend to infinity, then the relations between the particular integrals are the same as when p is not an integer; but if the series are supposed to terminate absolutely when the zero terms occur, the relations are quite different. As the finite portions of the particular integrals satisfy the differential equation, it is more natural to regard the series as terminating absolutely, and on this supposition the relations between the particular integrals exhibit a remarkable diversity of form, according as p is or is not an integer.

The second section contains what is believed to be a new form of the solution of (1) in the case of $p=$ an integer. It is shown that this equation is satisfied by the coefficient of h^{p+1} in the expansion of $e^{a\sqrt{(x^2+xh)}}$ in ascending powers of h. The six particular integrals given in the first section and the relations connecting them are also obtained by different expansions of this expression.

The third section contains the six particular integrals of (2) and (3), corresponding to those of (1), from which they are deduced.

The fourth section relates to the particular cases in which the differential equations admit of integration in a finite form. If a differential equation is satisfied by an infinite series, and if for certain values of a quantity involved in it the series terminates, then in this case we may present the integral in a different form by commencing the finite series at the other end and writing the terms in the reverse order. These reverse forms in the case of (1), (2), (3) are given in this section.

The fifth section contains the evaluations of the definite integrals—

$$\int_0^\infty e^{-x^m-\frac{a^2}{x^m}}dx, \qquad \int_0^\infty \frac{\cos bx}{(a^2+x^2)^n}dx,$$

where m denotes any real quantity and n any positive quantity. These integrals have been evaluated when m is of the form $\frac{-4i}{2i\pm1}$, and when n is a positive integer, but the general formulæ are, the author believes, new. The results exhibit changes of form similar to those referred to in the account of the contents of the first section. Certain formulæ of Cauchy's and Boole's are also considered in this section.

The sixth section, which is the longest in the memoir, relates to the different symbolic solutions of (1), (2), (3) in the cases in which they are integrable in finite terms. In this section these symbolic solutions are derived from the definite integrals considered in the fifth section; and the various symbolic theorems to which they lead, by comparing different forms of the results, are examined. A great many symbolic solutions of these differential equations have been given by Gaskin, R. L. Ellis, Boole, Lebesgue, Hargreave, Williamson, Donkin, and others, and these are briefly noticed and connected with one another.

The seventh section relates to the connexion between the results contained in this memoir and the formulæ of Bessel's Functions. The equation (1) may, as is well known, be transformed into Bessel's equation

$$\frac{d^2w}{dx^2}+\frac{1}{x}\frac{dw}{dx}+\left(1-\frac{\nu^2}{x^2}\right)w=0$$

by the substitutions $u=x^{\frac{1}{2}}w$, $p+\frac{1}{2}=\nu$, $a^2=-1$, so that the theorems relating to the solutions of (1) have analogues in the solutions of Bessel's equation.

The eighth section contains a list of writings which are closely connected with the subject of the memoir, with short accounts of their contents.

XXIII. "Sur la Surface de l'Onde, et Théorèmes relatifs aux Lignes de Courbure des Surfaces du Second Ordre." Par A. MANNHEIM. Communicated by THE PRESIDENT. Received June 16, 1881.

Après avoir présenté quelques remarques sur la surface de l'onde, je montrerai dans cette courte note, comment l'emploi d'une simple proposition de *géométrie cinématique* permet de trouver des résultats nouveaux et intéressants à propos d'un sujet déjà bien étudié.

§ I.

A un ellipsoïde donné de centre o, *on circonscrit des cônes dont une section principale est un angle droit: les sommets de ces cônes sont sur une surface de l'onde* [c].

Cette génération de la surface de l'onde, que j'ai communiquée à l'Académie des Sciences (26 Avril, 1880), va me servir de point de départ.

Appelons c le sommet d'un de ces cônes, ca, cb, les deux génératrices perpendiculaires l'une à l'autre dont le plan est une section principale de ce cône; a et b les points de contact de ces génératrices et de l'ellipsoïde donné.

Considérons les trois surfaces du second degré homofocales à l'ellipsoïde donné qui passent par le sommet c.

On sait que ces surfaces sont respectivement tangentes en c aux plans principaux du cône ou, ce qui revient au même, les normales à ces surfaces en ce point sont les axes principaux de ce cône.

Parmi ces surfaces il y a un ellipsoïde. Nous le désignerons par (E); il coupe la surface de l'onde, suivant une courbe E. La normale en c à cet ellipsoïde est la bissectrice $c\beta$ de l'angle droit $a\,c\,b$.

La normale en c à la surface de l'onde est, comme je l'ai démontré, la droite $c\mu$, qui joint le point c au milieu μ de la corde de contact $a\,b$.

Le plan $a\,c\,b$ est alors le plan normal en c à la courbe E; par suite la droite G élevée du point c perpendiculairement au plan $a\,c\,b$ est la tangente en c à cette courbe; et l'on sait que la droite G, l'un des axes principaux du cône circonscrit, est aussi l'un des axes de l'indicatrice de l'ellipsoïde (E) au point c.

Ceci est vrai pour un point quelconque de E; nous voyons alors que:

La surface de l'onde [c] *est coupée par un ellipsoïde* (E) *homofocal à l'ellipsoïde qui entre dans la définition précédente, suivant une ligne de courbure de cet ellipsoïde* (E).

Comme cette ligne de courbure est l'intersection de l'ellipsoïde (E) et d'un hyperboloïde homofocal, ce théorème s'applique aussi à cet hyperboloïde.

Considérons maintenant l'autre hyperboloïde homofocal, celui qui est tangent en c au plan $a\,c\,b$ ou, ce qui revient au même, dont la normale en c est la droite G. Appelons (c) l'intersection de cet hyperboloïde et de la surface de l'onde. Projetons l'ellipsoïde donné sur le plan $a\,c\,b$. Nous obtenons ainsi une ellipse de contour apparent, tangente en a et b aux côtés de l'angle droit $a\,c\,b$. Le centre de cette ellipse est un point de $c\mu$, mais ce centre est la projection du centre o de l'ellipsoïde donné donc *le plan* (G, $c\,\mu$) *contient le centre* o *de l'ellipsoïde donné.*

La droite G est la normale à l'hyperboloïde, et la droite $c\mu$ est la normale à la surface de l'onde. Le plan de ces deux droites est alors normal en c à l'intersection de (c) de ces deux surfaces. Mais, comme nous venons de le voir, ce plan contient o, donc co est normale à cette courbe d'intersection. Ainsi, les droites partant de o, et qui s'appuient sur (c), rencontrent cette courbe à angle droit; donc:

La courbe (c) *est une ligne sphérique.*

Les droites $c\mu$ et G étant perpendiculaires l'une à l'autre, l'hyperboloïde et la surface de l'onde se coupent en c à angle droit; et comme ceci est vrai pour un point quelconque de (c) nous voyons que :

Le long de la ligne sphérique (c), *la surface de l'onde et l'hyperboloïde se coupent à angle droit.*

Cette courbe sphérique (c) est du quatrième ordre; elle n'est pas alors l'intersection complète de l'hyperboloïde et de la surface de l'onde. La partie restante de cette intersection est une courbe du quatrième ordre, lieu du sommet d'angles droits tels que $a\ c\ b$, mais dont le plan est normal à l'hyperboloïde: cette courbe est alors une ligne de courbure de cette hyperboloïde.

Ce que nous disons pour cet hyperboloïde est applicable à l'autre, et l'on voit que :

Les hyperboloïdes homofocaux à l'ellipsoïde, qui entre dans la définition de la surface de l'onde, coupent chacun cette surface, suivant une ligne sphérique et une de leurs lignes de courbure.

La tangente en c à la courbe sphérique (c) est perpendiculaire au plan (G, $c\mu$), elle est alors perpendiculaire à G qui est la tangente à E. On voit donc que :

Les courbes telles que (c) *et* E *se rencontrent à angle droit.*

On peut dire aussi :

Les courbes sphériques (c) *suivant lesquelles la surface de l'onde* [c] *est coupée par des sphères concentriques ont, pour trajectoires orthogonales les courbes* E,* *suivant lesquelles cette surface est coupée par des ellipsoïdes homofocaux à celui qui entre dans la définition précédente.*

Le cône du second ordre, qui a pour sommet o et pour directrice (c), a pour plan tangent le long de oc un plan perpendiculaire au plan $(G, c\mu)$. Mais ce plan $(G, c\mu)$ est tangent au cône du second ordre dont la directrice est E.

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* Les courbes E sont les *courbes ellipsoïdales* de Lamé.

Donc :

Les cônes du second ordre de sommet o, *et qui ont pour directrices des courbes telles que* (c) *et* E, *se coupent à angle droit.*

On peut dire que la droite G est la projection de oc sur le plan tangent en c à la surface de l'onde, et qu'alors, au moyen de la projection de oc, on obtient la tangente en c à la ligne de courbure E. Appliquons cette remarque :

Le cône de sommet o, dont la directrice est (c), a pour plan tangent le long de oc, un plan perpendiculaire au plan $(o, c\mu)$ normal en c à la surface de l'onde. Ce plan tangent, en vertu d'un théorème connu, est alors le plan normal à la surface de l'onde au point γ, où le rayon oc rencontre cette surface. Il coupe le plan tangent en γ à la surface de l'onde, d'après la remarque précédente, suivant la tangente en ce point à une ligne de courbure de l'ellipsoïde homofocal à l'ellipsoïde donné, et qui contient γ. On voit ainsi que :

Le cône de sommet o, *et dont la directrice est* (c), *rencontre de nouveau la surface de l'onde, suivant une courbe telle que* E.

On démontre facilement la proposition inverse.

En rapprochant les résultats précédents de ceux trouvés par MM. W. Roberts et Massieu, au moyen de l'équation de la surface de l'onde en co-ordonnées elliptiques, on voit que l'ellipsoïde, qui entre dans ma définition de la surface de l'onde, fait partie des surfaces homofocales qui interviennent dans cette équation.

§ II.

Jusqu'à présent j'ai considéré une surface de l'onde et des ellipsoïdes homofocaux. Nous allons maintenant prendre un ellipsoïde fixe et une série de surfaces $[c']$ analogues à la surface de l'onde. Nous verrons que ces surfaces coupent aussi cet ellipsoïde suivant des lignes de courbure; nous obtiendrons alors une nouvelle génération des lignes de courbure des surfaces du second ordre. *Les surfaces* [c'] *sont le lieu des sommets de cônes circonscrits à un ellipsoïde donné, et dont une section principale est égale à un angle donné arbitrairement.*

Appelons $c'a'$, $c'b'$ les deux génératrices, qui forment une section principale du cône circonscrit de sommet c', comprenant entr'elles l'angle donné : les points a' et b' étant les points de contact de ces génératrices et de l'ellipsoïde donné. On peut dire que *la surface* [c'] *est le lieu du sommet d'un angle de grandeur constante* a' c' b', *circonscrit à l'ellipsoïde donné, et dont le plan est normal à cet ellipsoïde en chacun des points de contact* a' *et* b'.

En partant de cette définition et en faisant usage d'une simple proposition de *géométrie cinématique*, cherchons d'abord la normale en c' à la surface $[c']$.

Pour un déplacement de l'angle de grandeur constante $a'\,c'\,b'$, le foyer du plan de cet angle est à la rencontre f des normales élevées des points a' et b' à l'ellipsoïde donné. Comme la position de ce foyer est indépendante du déplacement de l'angle mobile, nous en concluons que *la droite* c′ f *est la normale en* c′ *à la surface* [c′].

La bissectrice de l'angle a′ c′ b′ *est toujours la normale en* c′ *à un ellipsoïde* (E) *homofocal à l'ellipsoïde donné et qui contient* c′. Le plan de ces deux normales, c'est-à-dire le plan de l'angle mobile, est alors le plan normal en c' à la ligne d'intersection de (E) et de $[c']$. La tangente à cette courbe d'intersection est alors, comme précédemment, l'un des axes principaux du cône de sommet circonscrit à l'ellipsoïde donné et aussi l'un des axes de l'indicatrice de (E) en c'. Ceci est vrai pour un autre point tel que c' ; nous avons alors ce théorème curieux :

Un angle de grandeur constante, circonscrit à un ellipsoïde donné et dont le plan est normal à cette surface en chacun des points de contact des côtés de cet angle, se déplace de façon que son sommet reste sur l'ellipsoïde(E) *homofocal à l'ellipsoïde donné : ce sommet décrit une ligne de courbure de* (E).*

Ce théorème subsiste si les côtés de l'angle mobile touchent respectivement des ellipsoïdes homofocaux, le plan de cet angle restant toujours normal à ces ellipsoïdes.

On peut encore dire inversement :

Par une tangente à une ligne de courbure d'un ellipsoïde on mène deux plans qui touchent respectivement un ellipsoïde homofocal à celui-ci : l'angle compris entre ces plans est de grandeur constante, quelle que soit cette tangente.

XXIV. "On certain Definite Integrals." No. 9. By W. H. L. RUSSELL, F.R.S. Received June 16, 1881.

Continuing the investigations given in the last paper, we have :—

$$\int_0^{\frac{\pi}{2}} \theta d\theta \{\epsilon^{2i\theta} f(\cos\theta\epsilon^{i\theta}) - \epsilon^{-2i\theta} f(\cos\theta\epsilon^{-i\theta})\} = \pi i \phi \tfrac{1}{2} \quad . \quad . \quad . \quad (176),$$

when $\phi(x) = \int dx\, f(x)$.

$$\int_0^{\pi} \theta d\theta \{\epsilon^{i\theta} f(\epsilon^{i(\theta+\pi)}) - \epsilon^{-i\theta} f(\epsilon^{-i(\theta+\pi)})\} = 2\pi i \phi(1) \quad . \quad . \quad (177),$$

when $\phi(x) = \int dx\, f(x)$ as before.

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* On peut remarquer que la ligne de courbure ainsi décrite rencontre toujours à angle droit le plan de l'angle mobile.

$$\int_0^\pi d\theta \frac{\sin^2 n\theta}{\sin^2\theta} = n\pi \quad \ldots\ldots \quad (178).$$

$$\int_0^\pi d\theta \frac{(2n-1)\sin\theta - \sin(2n-1)\theta}{2\sin^3\theta} = n(n-1)\pi \quad \ldots\ldots \quad (179).$$

$$\int_0^\pi d\theta \,.\, \frac{n^2\sin^2\theta - \sin^2 n\theta}{2\sin^4\theta} = \frac{n(n^2-1)\pi}{3} \quad \ldots\ldots \quad (180).$$

$$\int_0^\infty \frac{\sin^2 n\theta}{\theta\sin\theta} = \frac{n\pi}{2} \quad \ldots\ldots \quad (181).$$

$$\int_0^\omega d\theta \frac{(2n-1)\sin\theta - \sin(2n-1)\theta}{\theta\sin^2\theta} = n(n-1)\pi \quad \ldots\ldots \quad (182).$$

$$\int_0^\infty d\theta \,.\, \frac{n^2\sin^2\theta - \sin^2 n\theta}{\theta\sin^3\theta} = \frac{n(n^2-1)\pi}{3} \quad \ldots\ldots \quad (183).$$

$$\int_0^{\frac{\pi}{2}} d\theta \,.\, \frac{1-\cos n\theta\cos n\theta}{\sin^2\theta} = \frac{n\pi}{2} \quad \ldots\ldots \quad (184).$$

From integrals (178), (181), (184), we obtain the three following:—

$$\int_0^\omega d\theta \,.\, \frac{(f(1)-f(\epsilon^{2i\theta})) + (f(1)-f(\epsilon^{-2i\theta}))}{\sin^2\theta} = 4\pi f'(1) \quad \ldots \quad (185)$$

$$\int_0^\infty d\theta \frac{(f(1)-f(\epsilon^{2i\theta})) + (f(1)-f(\epsilon^{-2i\theta}))}{\theta\sin\theta} = 2\pi f'(1) \quad \ldots \quad (186).$$

$$\int_0^{\frac{\pi}{2}} d\theta \,.\, \frac{(f(1)-f(\cos\theta\,\epsilon^{i\theta}) + (f(1)-f(\cos\theta\epsilon^{-i\theta}))}{\sin^2\theta} = \pi f'(1) \quad . \quad (187).$$

$$\int_0^{\frac{\pi}{2}} dx\{{}^{2ix}f(\cos x\epsilon^{ix}) - \epsilon^{-2ix}f(\cos x\epsilon^{-ix})\} = 2i\phi(1) \quad \ldots\ldots \quad (188).$$

$$\int_0^{\frac{\pi}{2}} d\theta\{\epsilon^{2i\theta}f(\epsilon^{\theta i}\sin\theta) - \epsilon^{-2i\theta}f(\epsilon^{-\theta i}\sin\theta)\} = i\{\epsilon^{\frac{i\pi}{2}}\phi\epsilon^{\frac{i\pi}{2}} + \epsilon^{-\frac{i\pi}{2}}\phi\epsilon^{\frac{i\pi}{2}}\} \quad . \quad (189).$$

$$\int_0^{\frac{\pi}{2}} d\theta\{\epsilon^{2i\theta}f(\sin\theta\epsilon^{i\theta}) + \epsilon^{-2i\theta}f(\sin\theta\epsilon^{-i\theta})\} = \frac{\epsilon^{\frac{i\pi}{2}}\phi\epsilon^{\frac{2\pi}{2}} + \epsilon^{-\frac{i\pi}{2}}\phi\epsilon^{-\frac{i\pi}{2}}}{i} \quad . \ . \quad (190).$$

In the three last integrals $\phi(x)$ has the same signification as before.

Let $\rho = a + b\cos\theta + c\cos^2\theta + \ \ldots\ + \cos^n\theta$—

$$\int_0^{\frac{\pi}{2}} d\theta\{f(\rho\epsilon^{ni\theta})+f(\rho\epsilon^{-ni\theta})\}=\pi f\frac{1}{2^n} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (191),$$

if $\rho=a+b\sin\theta+\ .\ .\ .\ \sin^n\theta$, where $n=4m+1$.

$$\int_0^{\frac{\pi}{2}} d\theta\{f(\rho\epsilon^{ni\theta})-f(\rho\epsilon^{-ni\theta})\}=i\pi f\frac{1}{2^n} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (192).$$

Let α, β, be the roots of $x^2+2x+2=0$, then we have the following integrals:—

$$\int_0^\infty dx\{\epsilon^{\alpha x}f(\epsilon^{\alpha x})+\epsilon^{\beta x}f(\epsilon^{\beta x})\}=\phi(1) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (193),$$

$$\int_0^\infty dx\{\epsilon^{\alpha x}f(\epsilon^{\alpha x})-\epsilon^{\beta x}f(\epsilon^{\beta x})\}=i\phi(1) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (194).$$

We shall also be able to find:—

$$\int_0^\infty \frac{(a+bx)dx}{g+2hx+x^2}(f\epsilon^{ix})-f(\epsilon^{-ix})) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (195).$$

$$\int_0^\infty \frac{(a+bx)dx}{g+2hx+x^2}(f(\epsilon^{ix})+f(\epsilon^{-xi})) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (196).$$

Let $\phi_r(x)=\int\int\int\ .\ .\ .\ f(x)dx^r$, then—

$$\int_0^{\frac{\pi}{2}} \theta d\theta\{\epsilon^{4i\theta}f(\cos\theta\epsilon^{i\theta})-\epsilon^{-4i\theta}f(\cos\theta\epsilon^{-i\theta})\}=2i^3\pi\phi_2(\tfrac{1}{2}) \quad . \; . \; . \; . \quad (197).$$

$$\int_0^{\frac{\pi}{2}} \theta d\theta\{\epsilon^{6i\theta}f(\cos\theta\epsilon^{i\theta})-\epsilon^{-6i\theta}f(\cos\theta\epsilon^{-i\theta})\}=8i^5\pi\phi_3(\tfrac{1}{2}) \quad . \; . \; . \; . \quad (198).$$

$$\int_0^\pi \theta d\theta f(\sin\theta)=\frac{\pi}{2}\int_0^\pi d\theta f(\sin\theta) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (199).$$

$$\int_0^\infty \frac{d\theta}{\theta}\left\{f\frac{1}{\alpha+\epsilon^{-i\theta}}-f\frac{1}{\alpha+\epsilon^{-i\theta}}\right\}=i\pi\left\{f\frac{1}{1+\alpha}-f0\right\} \quad . \; . \; . \; . \; . \; . \quad (200).$$

$$\int_0^\pi d\theta\cos r\theta\left\{\frac{\epsilon^{-\theta i}}{\epsilon^{-\theta i}-\alpha}f\frac{1}{\epsilon^{-\theta i}-\alpha}+\frac{\epsilon^{\theta i}}{\epsilon^{\theta i}-\alpha}f\frac{1}{\epsilon^{\theta i}-\alpha}\right\}$$

$$=\pi\left\{A_0+A_1\frac{d}{d\alpha}+\frac{A_2}{1\,.\,2}\frac{d^2}{d\alpha^2}+\ .\ .\ .\right\}\alpha^r \quad . \quad (201),$$

where A_0A_1 . . . are the coefficients of the expansion of $f(x)$.

$$\int_0^\pi d\theta \sin r\theta \left\{ \frac{1}{\epsilon^{-\theta i}-\alpha} f \frac{1}{\epsilon^{-\theta i}-\alpha} - \frac{1}{\epsilon^{-\theta i}-\alpha} f \frac{1}{\epsilon^{-\theta i}-\alpha} \right\}$$

$$=2\pi \left\{ \mathrm{A}_0 + \mathrm{A}_1 \frac{d}{d\alpha} + \frac{\mathrm{A}_2}{1\,.\,2} \cdot \frac{d^2}{d\alpha^2} + \ldots \right\} \alpha^{r-1} \quad . \quad (202).$$

$$\int_0^\infty \frac{\theta d\theta}{1+\theta^2} \left\{ f\frac{1}{\epsilon^{-\theta i}-\alpha} - f\frac{1}{\epsilon^{-\theta i}-\alpha} \right\} = i\pi \left\{ f\frac{1}{\epsilon-\alpha} - f0 \right\} \quad . \quad . \quad . \quad . \quad (203).$$

$$\int_0^{\frac{\pi}{2}} \frac{d\theta}{\tan\theta} \left\{ f\frac{1}{p-i\,\mathrm{cotan}\,\theta} - f\frac{1}{p+i\,\mathrm{cotan}\,\theta} \right\} = i\pi \left\{ f\frac{1}{p+1} - f0 \right\} \quad . \quad (204).$$

$$\int_0^\infty \frac{dx\,.\,x}{q^2+x^2} \left\{ f\frac{1}{p-xi} - f\frac{1}{p+xi} \right\} = i\pi \left\{ f\frac{1}{p+q} - f0 \right\} \quad . \quad . \quad . \quad . \quad (205).$$

$$\int_0^\infty \frac{dx}{x} \left\{ f\frac{1}{\rho-ix} - f\frac{1}{\rho+ix} \right\} = i \left\{ f\frac{1}{\rho} - f0 \right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (206).$$

Formula (199) leads to some results, which may well be noticed separately.

$$\int_0^\pi \frac{d\theta\,.\,\theta}{a+b\sin\theta} = \frac{\pi}{2\sqrt{a^2-b^2}} \left\{ \pi - \frac{2}{\sqrt{a^2-b^2}} \tan^{-1} \frac{b}{\sqrt{a^2-b^2}} \right\} \quad . \quad . \quad (207).$$

This may be written (for a particular case) :—

$$\int_0^\pi \frac{d\theta\,.\,\theta}{1+2a\sin\theta+a^2} = \frac{\pi}{2(a^2-1)} \left\{ \pi - \frac{2}{a^2-1} \tan^{-1} \frac{2a}{a^2-1} \right\} \quad . \quad . \quad . \quad (208).$$

$$\int_0^\pi \frac{\theta d\theta}{\sqrt{a+b\sin\theta+\ldots e\sin^n\theta}} = \frac{\pi}{2} \int_0^\pi \frac{d\theta}{\sqrt{a+\ldots+e\sin^n\theta}} \quad . \quad . \quad . \quad (209).$$

Hence
$$\int_0^\pi \frac{\theta d\theta}{\sqrt{a+b\sin^2\theta}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (210),$$

can be expressed as an elliptic function.

The integral $\int_0^\infty \frac{\tan a\theta}{\theta} = \frac{\pi}{2}$ (stated to be due to Cauchy) which is proved in Gregory's examples, gives us

$$\int_0^\infty \frac{d\theta}{\theta} \left\{ \frac{1}{2^n} \mathrm{cotan}\, \frac{\theta}{2^n} - \mathrm{cotan}\,\theta \right\} = \frac{\pi}{2}\left(1-\frac{1}{2^n}\right) \quad . \quad . \quad (211).$$

Let (n) increase without limit, then this integral becomes—

$$\int_0^\infty \frac{d\theta}{\theta} \left\{ \frac{1}{\theta} - \mathrm{cotan}\,\theta \right\} = \frac{\pi}{2} \quad . \quad . \quad . \quad . \quad . \quad (212).$$

$$\int_0^\pi \frac{d\theta}{\epsilon^{5\alpha\cos\theta}-\epsilon^{3\alpha\cos\theta}+\epsilon^{\alpha\cos\theta}}=\tfrac{1}{2}\int_0^\pi\left(\epsilon^{\alpha\cos\theta}+\epsilon^{-\alpha\cos\theta}\right)d\theta \quad . \quad . \quad (213).$$

Since, in the equation of differences

$$u_{n+2}+pu_{n+1}+qu_n=0,$$

the ratio $\frac{u_{n+1}}{u_n}$ can always be expressed by continued fraction, and since if

$$u_n=\int_\alpha^\beta \frac{x^n}{\sqrt{a+bx+cx^{n2}}},$$

where β and α are the roots $a+bx+cx^2=0$,

$$u_n+\frac{2n-1}{2n}\cdot\frac{b}{c}u_{n-1}+\frac{n-1}{n}\frac{a}{c}u_{n-2}=0,$$

it is evident that the ratio of $\int_\alpha^\beta \frac{x^{r+1}}{\sqrt{a+bx+cx^2}}$ to $\int_\alpha^\beta \frac{x^r}{\sqrt{a+bx+cx^2}}$ can be expressed by a continued fraction. Similar treatment will apply to the integrals—

$$\int \frac{dx\,.\,x^n}{\sqrt[3]{a+bx+cx^2}} \quad . \quad . \quad (214), \qquad \int \frac{x^r dx}{\sqrt{(1-k^2x^2)(1-x^2)}} \quad . \quad . \quad (215),$$

$$\int \frac{x^n}{\sqrt[3]{(1-k^2x^2)(1-x^2)}} \quad (216), \qquad \int \frac{d\theta}{(a+b\cos\theta)^n} \quad . \quad . \quad . \quad . \quad (217),$$

$$\int \frac{d\theta}{(a+b\sin\theta)^n} \quad . \quad . \quad (218), \qquad \int \frac{d\theta}{(1-c^n\sin^2\theta)^{\frac{2n+1}{2}}} \quad . \quad . \quad . \quad (219),$$

and many other integrals.

XXV. "On the Influence of Coal-dust in Colliery Explosions. No. III." By W. GALLOWAY. Communicated by R. H. SCOTT, F.R.S. Received May 30, 1881.

(Abstract.)

The information that can be gleaned from a study of the three great colliery explosions of the past year, namely, Risca, Seaham, and Penygraig, appears to throw more light upon the question than anything that has been elicited experimentally either by myself or others since I last had the honour of addressing the Society on the same subject. These explosions took place on the 15th of July, the 8th of

September, and the 10th of December respectively, and they cost the lives of no fewer than 385 persons in all—Risca, 120; Seaham, 164; and Penygraig, 101.

For the purposes of the present paper I visited Risca Colliery on the 24th of October, and Seaham Colliery on the 24th of November, and I have had the rare opportunity of becoming thoroughly conversant with every circumstance connected with the Penygraig explosion, and with the events which immediately preceded and accompanied its occurrence.

A careful consideration of all the preceding details has irresistibly convinced me that, whatever may be the results of experiments with coal-dust made on a small scale, this is not a mere question of "adding fuel to the flame," of "aggravating the consequences of an explosion," of "spreading an explosion," or of "lengthening out the flame of firedamp," or that of a blown-out shot. Expressions of that kind not only fail to convey a true idea of the actual state of matters, but they are apt to be misleading. I might describe the impression upon my own mind to be somewhat as follows:—Given a well-ventilated or badly-ventilated dry mine, with moderately clean fine dry coal-dust lying everywhere all over the workings, and of any quality varying from a long-flaming house coal, like that of Blantyre, on the one hand, to a semi-bituminous steam coal, like that of Penygraig, on the other hand. Then if an explosion be begun at any point in one of the only three possible ways (1. By the coal-dust being raised and ignited by a blown-out or other shot; 2. By an explosive accumulation of firedamp and air being ignited by a blown-out or other shot; 3. By an explosive accumulation being ignited accidentally by a defective or other safety lamp, or in any other manner), and if it accumulates a certain degree of force without dying out, that force will go on increasing, and the explosion will inevitably extend to every open space in the workings, whatever may be their extent or ramifications. I make no mention of firedamp as necessary to the attainment of this result, with the exception of the comparatively small quantity required to create the first disturbance. It must not be imagined, however, that I lose sight of the latent firedamp contained in the air of every mine of this class. On the contrary, I am satisfied that that firedamp plays a most important part in every great explosion, and I have endeavoured strongly to urge this view in both of my previous papers.

XXVI. "The Molecular Volume of Solids." By E. WILSON. Communicated by Professor G. G. STOKES, Sec. R.S. Received May 19, 1881.

[Publication deferred.]

XXVII. "The Effects of certain modifying Influences on the Latent Period of Muscle Contraction." By Dr. G. F. YEO and Dr. CASH. Communicated by Dr. J. B. SANDERSON, F.R.S. Received June 15, 1881.

[Publication deferred.]

XXVIII. "On the Absorption of Gas by the Intestines and the Action of Carminatives upon it." By T. LAUDER BRUNTON, M.D., F.R.S., and THEODORE CASH, M.D. Received June 15, 1881.

[Publication deferred.]

XXIX. "On the Action of Alkali and Acid on Muscle: Frog and Rabbit." By T. LAUDER BRUNTON, M.D., F.R.S., and THEODORE CASH, M.D. Received June 15, 1881.

[Publication deferred.]

XXX. "On a New Form of Febrile Disease associated with the presence of an Organism distributed with Milk from the Oldmill Reformatory School, Aberdeen." By J. C. EWART, M.D., Professor of Natural History in the University of Aberdeen. Communicated by Dr. J. B. SANDERSON, F.R.S. Received June 16, 1881.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 17th.

"The Molecular Volume of Solids." By EDWARD WILSON, M.A. Communicated by Professor STOKES, Sec. R.S. Received May 19. Read June 16, 1881.

The object of the present paper is to trace the relation between the molecular volume of any solid substance and its chemical constitution. This subject has engaged the attention of several previous inquirers, notably of Kopp, Schröder, and Hermann, but a review of their labours will be postponed till an exposition of the principles of this paper has been given. Such an arrangement, it is thought, will very much facilitate a comparison between the views and results of the present writer, and those of his predecessors in the same field of inquiry.

The molecular volume of any solid substance may be defined to be the weight of the molecule divided by the specific gravity of the substance. The weight is known in terms of the standard unit, if the chemical composition be known. The specific gravity may be determined by experiment. Then we may form either of the equations—

$$\text{Volume of molecule} = \frac{\text{weight of molecule}}{\text{specific gravity}}.$$

$$\text{Specific gravity} = \frac{\text{weight of molecule}}{\text{volume of molecule}}.$$

Another definition might be given as follows:—Every solid and liquid substance may be conceived as made up of molecules separated from one another by intervals of space, and kept apart by the repulsive forces which each molecule exerts on the molecules adjacent to it. At a certain distance the molecules cease to repel, and beyond that distance they attract one another; consequently, any molecule may be regarded as situate at the centre of a sphere within which any similar molecule would be repelled. This sphere may be called the sphere of repulsion, and the volume of the molecule may be defined as the volume of its sphere of repulsion. This definition is more suitable to the gaseous forms of matter, where the molecular volume is determined by observations on the interdiffusion of gases according to the plan adopted by Professor Loschmidt of Vienna. It is easy to show that according to this definition the molecular volume of any solid substance is equal to its molecular weight divided by its specific gravity.*

Each atom which enters into the composition of a molecule has a known and invariable atomic weight, and the weight of the molecule is the sum of the weights of the component atoms. In like manner the simple *monatomic* molecule of any element must have a determinate and invariable volume if the element could be reduced to the monatomic condition, and this volume may be defined as its atomic volume. But an element does not carry its atomic volume unchanged into its compounds in the same way that it carries its atomic weight; indeed, it is not probable that an atom of a compound molecule has any volume at all (except, of course, in so far as the mere matter of the atom may have a volume) in the sense of occupying exclusively of other atoms a discrete portion of space. The whole volume of the molecule is shared by its constituent atoms in common, and no separate portion of its space can be assigned exclusively to any one particular atom; nevertheless, each atom of the molecule must play its part in the formation of the common volume, and therefore a certain proportion of that volume may be attributed to each atom in it.

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* *Vide* note.

The invariable and all-important atomic volumes defined in the preceding paragraph cannot be the subject of direct experimental investigation, except perhaps in the case of the few elements, such as mercury, cadmium, and zinc, which are known to be monatomic in the gaseous state at terrestrial temperatures, though all the elements may be conceived as capable of thus existing under suitable conditions. The atomic volumes must in the case of each element be deduced from a comparison of the specific gravities of the various compounds in which that element figures as a constituent. The first point to be aimed at is to discover, by means of comparisons, the values of these atomic volumes, because they of necessity form the only sound and rational basis of all speculations on the volumes of compound molecules. There exists no other firm ground or secure starting point; atomic volumes are to molecular volumes what atomic weights are to molecular weights.

When two or more atoms combine to form a chemical compound, a very intimate union of some sort takes place between the atoms, of the real nature of which we are, in the present state of science, profoundly ignorant; but at any rate, a new molecule is formed with a new volume, and the question arises as to what relations subsist between this new volume and the atomic volumes of the components of the molecule. This is the problem which it is sought to solve, and the answer, perhaps, may best be given by the enunciation of the two following propositions:—

(1.) When any number of *similar* atoms combine, the volume of the resulting molecule is equal to that of the uncombined atom.

(2.) When *dissimilar* atoms combine, the volume to be attributed to each atom is some submultiple or simple aliquot part of its atomic volume, and the resulting molecular volume is the sum of these.

The somewhat speculative character of the above views will not escape the notice of any one, but before entering upon an explanation of the tables which follow, the author thought it right to state the physical conceptions by which he was guided in his work. It must, however, be premised, as will appear more fully in the sequel, that these conceptions are provisional only, and should they hereafter prove erroneous, their invalidity would not affect the truth of the main result arrived at, which it is now proper to formulate in the shape of a general proposition as follows:—

(3.) Every element is capable of assuming different volumes in its various compounds, but these diverse volumes always bear to each other a simple numerical proportion, such as 1 : 2, 1 : 3, 2 : 3, &c., &c.

Attention must now be directed to the tables which follow, and which contain the evidence for the principles put forward in this paper.

Table I contains a list of the molecular or atomic volumes. The expression "molecular *or* atomic" is used as implying that the two values are identical, as indeed they must be by virtue of the principle enunciated in proposition (1). It will be observed that by virtue of the same principle equal molecular volumes are attributed to each of the allotropic forms of an element, the allotropism being supposed to consist in the different number of atoms contained in their respective molecules. With regard to the third column of the same table, it should be pointed out that, since chemists are possessed of no means of determining the number of atoms in the molecule of an element in the solid state, the molecular weights here assigned to the elements have been chiefly derived from a consideration of the volumes which the element is found to assume in the various substances of which it is a constituent. The atomic volumes of the elements whose specific gravities in the solid form are known, have been deduced from the specific gravities of such elements and their compounds conjointly, but the atomic volumes of such elements as hydrogen, oxygen, and nitrogen, whose specific gravities in the solid form are unknown, have been deduced from the specific gravities of their compounds alone, without the assistance to be derived from the specific gravity of the element itself. These last-mentioned compounds, however, are so numerous that their atomic volumes may be considered to be determined with greater accuracy than those of any of the other elements, with the exception of carbon.

It would be impossible to present the evidence in favour of the preceding propositions without an adequate notation, and, therefore, it is necessary to explain the system of notation that has been adopted. The atomic volume of each element is represented in the tables by the ordinary symbol used to denote that element, accented; whilst the submultiple of its atomic volume which the element assumes in a particular molecule is indicated by a suffix. Thus the atomic volume of potassium (90) is represented by K'; whilst K'_3 in an expression for the molecular volume of any substance containing potassium would mean that the volume to be ascribed to potassium in that particular molecule is found by dividing the number 90 by 3, and then multiplying the quotient by the number of atoms of potassium in the molecule. Let us take an illustration from Table (IX). The molecular weight of potassium sulphate is expressed by K_2SO_4; the expression for its molecular volume is $K'_6S'_6O'_4$, which means that the number K' (=90) has to be divided by 6 and the quotient multiplied by 2; the number S' (=96) has to be divided by 6; the number O'(=20) has to be divided by 4, and the quotient multiplied by 4, and that the sum of the resulting numbers (30+16+20=66) is the molecular volume of potassium sulphate. The specific gravity is then obtained by dividing the molecular weight by the molecular volume: thus the specific gravity of potassium sulphate$=\frac{174}{66}=2{\cdot}636$, which agrees very well with its observed value $=2{\cdot}640$.

Another method of notation might have been adopted which has the advantage of getting rid of the accent and exhibiting both the molecular weight and volume in one and the same formula. Thus both the molecular weight and volume of potassium sulphate might be expressed by $K_{\frac{2}{6}}S_{\frac{1}{6}}O_{\frac{4}{4}}$, if the numerators of the fractions are understood to represent the number of atoms and the denominators the submultiples of the atomic volumes.

It is now necessary to explain more in detail, how the fundamental numbers of column IV, Table I, have been obtained. In the first place it may be observed that these numbers are always some multiple of the *atomic* weight of the element divided by its specific gravity; but that it requires an examination of the compounds of the element to determine what this multiple ought to be. Let us take one or two illustrations. The atomic weight of sodium divided by its specific gravity is 24, and an examination of the compounds of sodium discloses the fact that this element assumes in its compounds most frequently the volumes 8 and 12, and occasionally 24: the number 2, therefore, is the proper multiple in this case, and the fundamental number to be assigned to sodium is 48. Again, the atomic weight of iodine divided by its specific gravity is 25·6, but the most frequent volume of iodine in its compounds is 32 and, less frequently $21\frac{1}{3}$: the number 5 therefore is its proper multiple, and the fundamental number to be assigned to iodine is 128. For $25{\cdot}6\times5=128$, whilst 32 and $21\frac{1}{3}$ are respectively one-fourth and one-sixth of the same number. One more instance, perhaps, will suffice. The atomic weight of boron divided by its specific gravity is about =4, whilst its compound volumes are 7 and 14: whence 7 is the proper multiple and 28 its fundamental number.

Ammonium (NH_4) and cyanogen (CN) may be treated as simple elements, having as fundamental numbers the sum of the fundamental numbers of their constituents, viz., $N'+4H'=24+32=56$ and

$C'+N'=32+24=56$ respectively. The meaning of this is that the components of these radicles always undergo a like condensation. Water of crystallisation has a volume $H'_4O'_2=14$, whilst that of ammonia in ammonia-compounds is $N'_2H'_4=18$.

The tables which accompany this paper will be found to contain pretty strong evidence of the truth of a conjecture first made by Kopp with regard to oxygen, viz., that an element in one and the same compound may undergo different condensations if it enters into the composition of two *distinct* radicles. Hydrated ammonium sulphate, $(NH_4)_2SO_4.H_2O$, affords a good illustration of this, for its molecular volume is $(NH_4)'_3S'_6O'_4H'_4O'_2$, and it will be observed that the hydrogen in the ammonium radicle is condensed to one-third, whilst in the water of crystallisation it is condensed to one-fourth, and again the oxygen in the acid radicle is condensed to one-fourth, whilst in the water it is only one-half.

The following circumstance is well worth consideration. Many substances, having the same chemical composition, appear to possess two distinct specific gravities, and therefore different molecular volumes; a good instance of this is to be found in mercuric sulphide, (HgS), which, as cinnabar, has a specific gravity of about 9·0, but in its amorphous state, a specific gravity of about 7·6, corresponding to molecular volumes $Hg'_6S'_6$ and $Hg'_4S'_6$ respectively: (HgS) has also sometimes a specific gravity intermediate between these limits, indicating an admixture of the two states. These mixtures are rather puzzling to any theory of molecular volumes, just as the densities of gases at temperatures when they are undergoing dissociation appear to be anomalous according to Avogadro's law. Perhaps these substances might not inappropriately be called bivolumetric or disteric bodies; a few examples of such compounds are given in the following table:—

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Silicic dioxide (crystalline)............	SiO_2	$Si'_4O'_8$	2·688	2·690
Silicic dioxide (amorphous)............	,,	$Si'_4O'_4$	2·200	2·200
Zirconium dioxide....	ZrO_2	$Zr'_4O'_8$	5·655	5·624
,, ,, ...	,,	$Zr'_4O'_4$	4·588	4·35—4·90
Titanium dioxide (rutile).............	TiO_2	$T'_2O'_8$	4·256	4·250
Titanium dioxide (anatase)	,,	$Ti'_4O'_6$	3·875	3·890
Cobalt sesquioxide....	Co_2O_3	$Co'_4O'_4$	5·627	5·600
,, ,, ...	,,	$Co'_4O'_3$	4·811	4·814

There is one more circumstance deserving mention, and that is, the invariability of the volume of the constituents of the acid radicle in salts of the acid; for instance, in upwards of 60 sulphates, the radicle SO_4 has the same volume $S'_6O'_4=36$. It is true, the carbonates appear to form an exception to this rule, the radicle CO_3 having in some cases the volume $C'_4O'_4=23$, and sometimes the volume $C'_8O'_4=19$, and curiously enough, the oxalates follow suit in indicating two volumes for the radicle C_2O_4, viz., $C'_4O'_4=36$, and $C'_8O'_4=28$.

The observed specific gravities, with a few exceptions, have been taken from Clarke's "Constants of Nature," being No. 255 of the Smithsonian Miscellaneous Collections, a very compact and useful volume, which contains almost all the determinations that have been made by observers in all parts of the world.

We may now proceed to point out the very fair agreement between results obtained on the present theory and those reached by Professor Loschmidt by quite a different method, namely, by observations on the interdiffusion of gases. A simple inspection of the following table will show that the molecular volumes obtained on the principles of this theory agree very well with those arrived at by Loschmidt.

Comparison of the results of the present theory with those obtained by Professor Loschmidt from his experiments on the inter-diffusion of gases.

Substance.	Molecular volume.	Present theory.	Loschmidt.	
			I.	II.
H_2	H'_2	8	7	7
CO	$C'_2O'_2$	26	25	25
N_2	N'_2	24	26	24
NO	$N'_2O'_2$	22	24	23
O_2	O'_2	20	22	21
HCl	$H'_2Cl'_2$	26·5	26·3	26·3
Cl_2	Cl'_2	45	45·6	45·6
H_2O	$H'_2O'_2$	18	18	18
H_2S'	$H'_2S'_4$	32	33	33
CO_2	$C'_2O'_2$	36	36	35
N_2O	$N'_2O'_2$	34	37	35
SO_2	$S'_4O'_2$	44	48	48
NH_3	$N'_2H'_2$	24	23·5	22·5
CH_4	$C'_2H'_2$	32	35	28
C_2N_2	$C'_2N'_2$	56	54	56

Kopp's labours on molecular volumes were devoted chiefly to liquid substances, which are beyond the scope of this inquiry. The following brief account of his views on the molecular volumes of solids is derived from Miller's "Chemistry." Kopp supposes that, in the case of the oxides, oxygen has three distinct volumes, 16, 32, and 64 (his numbers are adapted to the atomic weight of oxygen=100), which are in the simple numerical proportion 1, 2, and 4, but he never extends this principle to the other elements, unless it be, perhaps, to chlorine and the lighter metals; for, in the chlorides, the two values which he assigns to chlorine, 196 and 245, are in the ratio of 4 : 5; but then it is necessary to assume new volumes for the metals, which, like potassium, sodium, calcium, and magnesium, undergo condensation in the act of combining; and the volumes thus assumed for them exhibit no simple relation to the metals in an uncombined state. To the acid radicle SO_4 in the sulphates two volumes are likewise assigned, 186 and 236, which bear no simple relation to each other, and are not derived from the constituents of the radicle. To the radicles CrO_4, CO_3, and NO_3, in the chromates, carbonates, and nitrates, are assigned the volumes 228, 151, and 179, but then it does not appear that any connexion is traced between these numbers and the components of the respective radicles.

Schröder propounds the following principle:—"In every solid compound the volume measure (volume-maas) or the stere, of one of its elements, which, through the forces acting during crystallisation, determines all the other components and respective constituents, causes equal volume measures to take up equal steres. In other words, one of the elements assimilates all the others."

The number of atoms of each element in a compound is indicated in the ordinary manner by a whole number placed to the right of the under side of the symbol, and the number of its steres by a whole number to the right of the upper side. The stere is distinguished by an overstroke, and the observed and calculated volumes by a similar understroke. The element in a compound which determines the stere is also indicated by an overstroke; thus metallic silver is $\overline{Ag^2_1}=2\times\overline{5\cdot14}=\underline{10\cdot28}$, observed volume$=\underline{10\cdot28}$. Again, the chloride, bromide, and iodide of silver are represented thus:—

$$\overline{Ag^2_1}Cl^3_1=5\times\overline{5\cdot14}=\underline{25\cdot70}\text{ obs. vol. }=\underline{25\cdot70}.$$

$$\overline{Ag^2_1}Br^4_1=6\times\overline{5\cdot14}=\underline{30\cdot81}\quad\text{,,}\quad=\underline{30\cdot81}.$$

$$\overline{Ag^2_1}I^6_1=8\times\overline{5\cdot14}=\underline{41\cdot12}\quad\text{,,}\quad=\underline{41\cdot12}.$$

From this it is seen that in all these compounds the silver stere dominates.

Mercury has a stere$=5\cdot52$. Thus:—

Mercurous oxide$=\overline{Hg^5_2}O^2_1=7\times\overline{5\cdot52}=\underline{38\cdot64}$ obs. vol. $\underline{38\cdot64}$.

Mercuric oxide$=\overline{Hg^5_2}O^2_2=7\times\overline{5\cdot52}=\underline{38\cdot64}=2\times\underline{19\cdot38}$ obs. vol. $\underline{19\cdot32}$.

Amorphous black cinnabar$=\overline{\mathrm{Hg}_2^5}\mathrm{S}_2^6=11\times\overline{5{\cdot}52}=60{\cdot}72=2\times\underline{30{\cdot}36}$ obs. vol. $\underline{30{\cdot}36}$.

Red rhombohedric cinnabar$=\mathrm{Hg}_2^5\overline{\mathrm{S}_2^6}=11\times\overline{5{\cdot}30}=58{\cdot}30=2\times\underline{29{\cdot}10}$ obs. vol. $\underline{29{\cdot}10}$.

The black cinnabar is distinguished from the red by the fact that, in the former, the mercury stere dominates, whilst in the latter it is the sulphur$=\overline{5{\cdot}30}$.

The mercury in its chlorides and bromides, and also in the cyanide is present as Hg_1^3, and not as Hg_2^5, thus :—

Mercurous chloride$=\overline{\mathrm{Hg}_1^3}\mathrm{Cl}_1^2=6\times\overline{5{\cdot}52}=\underline{33{\cdot}12}$ obs. vol.$=\underline{33{\cdot}12}$.

„ bromide$=\overline{\mathrm{Hg}_1^3}\mathrm{Br}_1^4=7\times5{\cdot}52=\underline{38{\cdot}64}$ „ „ $=\underline{38{\cdot}64}$.

Mercuric chloride $=\overline{\mathrm{Hg}_1^3}\mathrm{Cl}_2^6=9\times\overline{5{\cdot}52}=\underline{49{\cdot}68}$ „ „ $=\underline{49{\cdot}68}$.

„ bromide$=\overline{\mathrm{Hg}_1^3}\mathrm{Br}_2^8=11\times\overline{5{\cdot}52}=\underline{60{\cdot}72}$ „ „ $=\underline{60{\cdot}72}$.

„ cyanide$=\overline{\mathrm{Hg}_1^3}\mathrm{Cy}_2^9=12\times\overline{5{\cdot}52}=\underline{66{\cdot}24}$ „ „ $=\underline{66{\cdot}24}$.

Manganese oxides and silicates.—Metalic manganese has, according to John, the volume $6{\cdot}9=\frac{1}{2}$ magnesium.

$$\mathrm{Mn}_4^5=5\times\overline{5{\cdot}52}=27{\cdot}6=4\times\underline{6{\cdot}9}\text{ obs. vol.}=\underline{6{\cdot}9}.$$

Pyrolusite$=\mathrm{Mn}_4^5\mathrm{O}_8^8=13\times\overline{5{\cdot}52}=71{\cdot}76=4\times\underline{17{\cdot}94}$ obs. vol.$-17{\cdot}8-18{\cdot}0$.

Manganite is isomorphous with göthite and diaspore.

The molecular volumes of these bodies are as follows :—

Diaspore$=\overline{\mathrm{Al}_2^2}\mathrm{H}_2^1\mathrm{O}_4^4=7\times\overline{5{\cdot}14}=\underline{35{\cdot}98}$ obs. vol.$=\underline{35{\cdot}98}$.

Manganite$=\mathrm{Mn}_4^5\mathrm{H}_4^2\overline{\mathrm{O}_8^8}=15\times\overline{5{\cdot}40}=81=2\times\underline{40{\cdot}5}$ obs. vol.$=\underline{40{\cdot}5}$.

Göthite$=\mathrm{Fe}_4^5\mathrm{H}_4^2\overline{\mathrm{O}_8^8}=15\times\overline{5{\cdot}40}=81=2\times\underline{40{\cdot}5}$ obs. vol.$=\underline{40{\cdot}5}$.

In diaspore, therefore, the aluminium stere dominates, but in manganese and göthite the oxygen stere$=\overline{5{\cdot}40}$.

All the other oxides of manganese contain the manganese as Mn_2^3, thus :—

Manganous oxide$=\overline{\mathrm{Mn}_2^3}\mathrm{O}_2^2=5\times\overline{5{\cdot}52}=2\times\underline{13{\cdot}80}$ obs. vol.$=\underline{13{\cdot}80}$.

Braunite „ $=\overline{\mathrm{Mn}_2^3}\mathrm{O}_3^3=6\times\overline{5{\cdot}52}=\underline{33{\cdot}12}$ obs. vol.$=\underline{33{\cdot}12}$.

Hausmannite „ $=\overline{\mathrm{Mn}_6^9}\mathrm{O}_8^8=17\times\overline{5{\cdot}52}=\underline{93{\cdot}82}=2\times\underline{46{\cdot}91}$ obs. vol. $=\underline{47{\cdot}10}$.

In the manganese silicates, the manganese has the condensation

Mn^3_2, and the silicic acid the same volume constitution as quartz, viz., $\text{Si}^2_1\text{O}^2_2$, thus:—

$$\text{Tephroite}=\text{Mn}^3_2\text{Si}^2_1\overline{\text{O}}{}^4_4=9\times\overline{5\cdot4}=\underline{48\cdot6}\ \text{obs. vol.}=\underline{48\cdot6}.$$

$$\text{Paisbergite}=\overline{\text{Mn}}{}^6_4\text{Ca}^1_1\text{Si}^{10}_5\text{O}^{15}_{15}=32\times\overline{5\cdot52}=\underline{176\cdot64}\ \text{obs. vol.}=\underline{176\cdot64}.$$

In the first of these minerals the oxygen stere dominates, whilst in the latter it is the manganese stere.

Hermann, examining a series of oxides having the form RO, finds that the volume of the oxygen in them=5, which he designates as its normal value. The volume (5) is probably correct, but why this particular volume should be dignified by the term normal, it is not so easy to see. The volume of a monatomic molecule of oxygen is the only one entitled to be called normal if it could be got at, and is probably=20. He then infers that the specific gravity of solid oxygen is $\frac{16}{5}=3\cdot2$. It is unnecessary to speculate on what is the specific gravity of solid oxygen, but it may be remarked, in passing, that solid oxygen cannot have less than two atoms in its molecule, and that its molecular volume is probably 20, which would give $\frac{32}{20}=1\cdot6$ as its probable specific gravity. Hermann then proceeds to assign this volume (5) to the oxygen of water, H_2O, the molecular volume of which=18, and then deducting (5) from (18) he obtains 13 for the volumes of the two hydrogen and 6·5 for a single atom of hydrogen. It is submitted that the tables which accompany this paper contain abundant evidence that oxygen contributes (10) to the molecular volume of water, and the hydrogen atoms (4) each. For in nearly a hundred compounds containing hydrogen, the hydrogen atom is never found with a volume greater than (4), but in a great number of cases with that volume. The next step is to determine the volume of nitrogen from a consideration of the density of fluid ammonia which is taken as ·629. Dividing the molecular weight of ammonia (17) by ·629 gives (27) as its molecular volume. Then deducting from (27) $3\times6\cdot5$ for the three hydrogen atoms, leaves 7·5 for the volume of nitrogen. The same observation may be applied to this determination as to the previous one, that hydrogen never has so great a volume as 6·5. According to the system of this paper, the molecular volume of ammonia is 24—Loschmidt makes it 23·5—the nitrogen contributing 12 and the three atoms of hydrogen (4) each, and therefore, though the density of liquid ammonia may be ·629 at a certain temperature, the probable specific gravity of solid ammonia is $\frac{17}{24}=\cdot708$. The density of liquid ammonia at $-10^\circ\cdot7$ C. has been found to be as high as ·650. By such methods as the above, and others, Hermann determines the normal volumes of the elements, but he supposes the non-metallic elements and the lighter metals capable of assuming other volumes than the normal ones in their compounds, though not so the heavy metals. For instance, taking unity to represent the normal volume, oxygen may assume the following volumes :—$\frac{1}{2}$, 1, $1\frac{1}{3}$, $1\frac{1}{2}$, $1\frac{2}{3}$, 2 and 3; sulphur, $\frac{1}{2}$, $\frac{3}{4}$, $\frac{7}{8}$, 1 and $1\frac{1}{2}$; and the halogens, $\frac{1}{3}$, $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{7}{8}$, $1\frac{1}{4}$.

In conclusion, it may be remarked that the tables lend their chief support to proposition (3). Propositions (1) and (2) must be considered more hypothetical, but the writer wished to place them on record, because, if true, they would afford a physical interpretation to the fundamental numbers of column (IV) Table I. In proposition (1) also is to be found an explanation of the allotropism of the elements, carbon, silicon and phosphorus, based as it is upon the supposition that these allotropic forms contain a different number of atoms in the nucleus of their molecules with the same molecular volumes, and there are independent physical reasons, which render it not improbable that the accumulation of similar atoms in the nucleus of a molecule would not alter its volume, whatever be the law of force in action. The question is perhaps not altogether beyond the reach of direct experimental investigation. For if experiments on interdiffusion could be carried on at the temperature of gaseous mercury, the determination of the molecular volume of *monatomic* mercury, by the method of Loschmidt, would shed a very great deal of light on the subject.

Note.—Let V be the volume of any solid or liquid substance, W its weight, S its specific gravity, then

$$S=\frac{W}{V}.$$

The unit of volume being the volume of the unit of weight of the standard substance.

For distinctness we will take the hydrogen atoms as the unit of weight, and water at its greatest density as the standard substance. Then

$$\text{Volume of molecule of water}=\frac{\text{weight of molecule}}{\text{specific gravity of water}}=\frac{H_2O}{1}=18,$$

or the unit of volume is $\frac{1}{18}$ part of the volume of the molecule of water.

Suppose the substance to contain n molecules, then—

$$S=\frac{\frac{W}{n}}{\frac{V}{n}}.$$

Now $\frac{W}{n}$ is the weight of one molecule, and we may define $\frac{V}{n}$ as the volume of one molecule.

But $$\frac{V}{n}=\frac{\text{volume of molecule of substance}}{\frac{1}{18}\text{ volume of molecule of water}}.$$

Consequently, the numerical value of $\frac{V}{n}$ would not be altered if we took for our definition of molecular volume any volume bearing a fixed proportion to the volume first taken as such definition. The numerator and denominator of the fraction (1) would both be increased or diminished in the same proportion. Now, inasmuch as the molecules of any substance will pack themselves as closely as their mutual repulsions will allow, it follows from geometry that the space $\frac{V}{n}$ is proportional to the sphere of repulsion of the molecule of the substance.

Hence we may define the volume of a molecule as the volume of its sphere of repulsion without affecting the equation.

$$\text{Specific gravity of substance}=\frac{\text{weight of molecule}}{\text{volume of molecule}}.$$

Table I.

Element.	Atomic weight.	Molecular weight.	Molecular or atomic volume.	Calculated sp. gr.	Observed sp. gr.
H	1	..	8		
O	16	..	20		
N	14	..	24		
F	19	..	24		
Cl.	35·5	..	45		
Br	80	..	88		
I.	127	I_5	128	4·961	4·950
C (diamond)	12	C_9	32	3·375	3·350
C (graphite)	"	C_6	32	2·250	2·250
C (lamp-black)	"	C_5	"	1·875	1·885
B (adamantine)	11	B_7	28	2·750	2·680
Si (graphitoidal)	28·5	Si_6	70	2·443	2·490
Si (amorphous)	"	Si_5	"	2·035	2·000
P (red)	31	P_7	102	2·127	2·140
P (yellow)	"	P_6	"	1·823	1·830
P (white)	"	P_5	"	1·519	1·515
S	32	S_6	96	2·000	2·050
K.	39	K_2	90	·866	·865
Na	23	Na_2	48	·958	·970
Ba	137		92		
Sr.	87·5	Sr_2	70	2·500	2·500
Ca	40	Ca_2	50	1·600	1·600
Mg	24	Mg_2	27	1·777	1·750
Al	27·5	Al_3	32	2·578	2·600
Zr	89·6	Zr_3	66	4·073	4·150
Ag	108	Ag_4	43	10·047	10·428
Cd	112	Cd_4	52	8·619	8·600
Zn	65	Zn_4	37	7·027	6·8—7·2

Elements.	Atomic weight.	Molecular weight.	Molecular or atomic volume.	Calculated sp. gr.	Observed sp. gr.
Cu	63·5	Cu_4	29	8·760	8·800
Hg	2·00	Hg_4	58	13·793	13·590
Pb	207	Pb_4	74	11·200	11·445
Ti	50	Ti_6	58	5·173	5·300
Sn	118	Sn_4	66	7·151	7·280
Sb	122	Sb_4	72	6·777	6·700
Bi	208	Bi_4	84	9·904	9·880
Ni	59	Ni_4	29	8·138	8·500
Co	59	Co_4	29	8·138	8·500
As	75	As_6	76	5·921	5·900
Cr	52·5	Cr_6	45	7·000	7·014
Fe	56	Fe_6	43	7·814	7·800
Mn	55	Mn_6	43	7·697	7·0—8·0
Pt	197·4	Pt_6	56	21·150	21·150

Table II.—Oxides.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Barium oxide	BaO	$Ba'_4O'_4$	5·464	5·456
Strontium "	SrO	$Sr'_4O'_4$	4·600	4·611
Calcium "	CaO	$Ca'_4O'_4$	3·200	3·180
Magnesium "	MgO	$Mg'_4O'_4$	3·404	3·2—3·6
Zinc "	ZnO	$Zn'_4O'_4$	5·684	5·684
Cadmium "	CdO	$Cd'_4O'_4$	7·111	6·950
Cupric "	CuO	$Cu'_4O'_4$	6·490	6·500

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Mercuric oxide	HgO	$Hg'_4O'_4$	11·077	11·100
Lead „	PbO	$Pb'_4O'_4$	9·489	9·500
Potassium „	K_2O	$K'_6O'_4$	2·685	2·660
Sodium „	Na_2O	$Na'_6O'_4$	2·952	2·870
Silver „	Ag_2O	$Ag'_4O'_2$	7·365	7·250
Cuprous „	Cu_2O	$Cu'_4O'_2$	5·836	5·750
Mercurous „	Hg_2O	$Hg'_4O'_2$	10·666	10·680
Silicic dioxide (amorphous)	SiO_2	$Si'_4O'_4$	2·200	2·200
„ „ (crystalline)	SiO_2	$Si'_4O'_8$	2·688	2·690
Titanium „ (rutile)	TiO_2	$Ti'_4O'_8$	4·256	4·250
„ „ (anatase)	TiO_2	$Ti'_4O'_6$	3·875	3·890
Zirconium „	ZrO_2	$Zr'_4O'_8$	5·655	5·624
„ „	ZrO_2	$Zr'_4O'_4$	4·588	4·35—4·90
Lead „	PbO_2	$Pb'_4O'_6$	9·497	9·500
Manganese „	MnO_2	$Mn'_4O'_6$	5·000	5·026
Stannic „	SnO_2	$Sn'_4O'_8$	6·976	6·960
Chromium trioxide	CrO_3	$Cr'_2O'_4$	2·680	2·674
Arsenic sesquioxide	As_2O_3	$As'_4O'_4$	3·733	3·738
Antimony „	Sb_2O_3	$Sb'_4O'_4$	5·725	5·778
Bismuth „	Bi_2O_3	$Bi'_4O'_4$	8·140	8·200
Cobalt „	Co_2O_3	$Co'_4O'_4$	5·627	5·600
„ „	Co_2O_3	$Co'_4O'_3$	4·811	4·814
Nickel „	Ni_2O_3	$Ni'_4O'_3$	4·811	4·814
Boron „	B_2O_3	$B'_2O'_6$	1·842	1·830
Aluminium „	Al_2O_3	$Al'_4O'_6$	3·953	3·950
Manganese „	Mn_2O_3	$Mn'_4O'_4$	4·331	4·325
Ferric „	FeO_3	$Fe'_4O'_6$	5·080	5·121
Antimony tetroxide	Sb_2O_4	$Sb'_4O'_8$	6·695	6·615
Antimonic oxide	Sb_2O_5	Sb_4O_8	6·680	6·600
Minium	Pb_3O_4	Pb_4O_4	9·073	9·080

Table III.—Fluorides.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Sodium fluoride	NaF	$Na'_6F'_3$	2.625	2·600
Potassium „	KF	$K'_6F'_3$	2·521	2·454
Calcium „	CaF_2	$Ca'_3F'_6$	3·162	3·162
Barium „	BaF_2	$Ba'_3F'_6$	4·526	4·580
Aluminium „	Al_2F_6	$Al'_4F'_6$	3·018	3·065
Hydro ammonic „	$(NH_4)HF_2$	$(NH'_4)_2H'_3F'_3$	1·221	1·211
Potassio titanic „	K_2TiF_6	$K'_4Ti'_6F'_6$	3·076	3·079*
Barium silicofluoride	$BaSiF_6$	$Ba'_3Si'_6F'_6$	4·213	4·279
Ammonium „	$(NH_4)_2SiF_6$	$(NH_4)'_2Si_6F'_6$	1·946	1·970
Sodium „	Na_2SiF_6	$Na'_6Si'_4F'_4$	2·710	2·754
Potassium „	K_2SiF_6	$K'_6Si'_4F'_4$	2·641	2·664
Ammonium stannofluoride	$(NH_4)_2SnF_6$	$(NH_4)'_2Sn'_6F'_6$	2·966	2·887
Potassium zircofluoride	K_2ZrF_6	$K'_4Zr'_6F'_6$	3·520	3·582
„ titanofluoride	$K_2TiF_6.H_2O$	$K'_6Ti'_3F'_6H'_4O'_2$	2·977	2·992
Copper silicofluoride	$(CuSiF_6)_2 13H_2O$	$Cu'_3Si'_3F'_6H'_4O'_2$	2·182	2·157

* In Clarke's "Constants of Nature," this is given as 2·079, but surely this is a misprint for 3·079; for it is highly improbable that the sp. gr. of K_2TiF_6 is less than that of its hydrate, $K_2TiF_6H_2O = 2·992$, or of $K_2SiF_6 = 2·664$.

Table IV.

Chlorides.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
a. Simple Chlorides.				
Potassium chloride	KCl	$K'_4Cl'_3$	1·986	1·994
Sodium "	$NaCl$	$Na'_4Cl'_3$	2·166	2·148
Silver "	$AgCl$	$Ag'_4Cl'_3$	5·572	5·567
Lead "	$PbCl_2$	$Pb'_4Cl'_3$	5·732	5·742
Barium "	$BaCl_2$	$Ba'_4Cl'_3$	3·961	3·886
Strontium "	$SrCl_2$	$Sr'_3Cl'_3$	2·971	2·960
Calcium "	$CaCl_2$	$Ca'_3Cl'_3$	2·330	2·362
Mercuric "	$HgCl_2$	$Hg'_3Cl'_3$	5·493	5·420
Mercurous "	Hg_2Cl_2	$Hg'_3Cl'_3$	6·821	6·946
Ammonium "	$(H_4N)Cl$	$(H_4N)'_3Cl'_3$	1·589	1·578
Magnesium "	$MgCl_2$	Mg'_2Cl_3	2·183	2·177
Cupric "	$CuCl_2$	$Cu'_2Cl'_3$	3·022	3·054
Cuprous "	Cu_2Cl_2	Cu_2Cl_3	3·355	3·376
Zinc "	$ZnCl_2$	Zn_2Cl_3	2·804	2·753
Cobalt "	$CoCl_2$	$Co'_2Cl'_3$	2·921	2·937
Manganese "	$MnCl_2$	Mn'_2Cl_3	2·448	2·480
Ferrous "	$FeCl_2$	Fe_2Cl_3	2·467	2·529
b. Double Chlorides.				
Potassium zincochloride	K_2ZnCl_4	$K'_4Zn'_2Cl'_3$	2·307	2·297
Ammonium "	$(NH_4)_2ZnCl_4$	$(NH_4)'_2Zn'_2Cl'_3$	1·806	1·72 —1·879
Potassium platinochloride	K_2PtCl_6	$K'_6Pt'_4Cl'_3$	3·645	3·386—3·694
Ammonium "	$(NH_4)_2PtCl_6$	$(NH_4)'_3Pt_3Cl_3$	3·016	3·009

Table IV (*continued*).

c. Hydrated Chlorides.

Substance.		Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Calcium	chloride	$CaCl_2.6H_2O$	$Ca'_3Cl'_3H'_4O'_2$	1·676	1·680
Strontium	,,	$SrCl_2.6H_2O$	$Sr'_3Cl'_3H'_4O'_2$	1·941	1·921
Barium	,,	$BaCl_2.2H_2O$	$Ba'_4Cl'_3H'_4O'_2$	3·031	3·052
Cobaltous	,,	$CoCl_2.6H_2O$	$Co'_2Cl'_3H'_4O'_2$	1·852	1·840
Cupric	,,	$CuCl_2.2H_2O$	$Cu'_3Cl'_3H'_4O'_2$	2·518	2·535
Magnesium	,,	$MgCl_2.6H_2O$	$Mg'_2Cl'_3H'_4O'_2$	1·592	1·562
Stannous	,,	$SnCl_2.2H_2O$	$Sn'_3Cl'_3H'_4O'_2$	2·812	2·759
Platinic	,,	$PtCl_4.8H_2O$	$Pt'_2Cl'_3H'_4O'_2$	2·417	2·431
Potassium iron	,,	$K_2FeCl_4.2H_2O$	$K'_4Fe'_4Cl'_3H'_4O'_2$	2·170	2·162
,, copper	,,	$K_2CuCl_4.2H_2O$	$K'_6Cu'_2Cl'_3H'_4O_2$	2·411	2·410
Ammonium	,,	$(NH_4)_2CuCl_4.2H_2O$	$(NH_4)'_3Cu'_2Cl'_3H'_4O'_2$	1·984	1·977
Potassium tin	,,	$K_2SnCl_4.3H_2O$	$K'_6Sn'_3Cl'_3H'_4O_2$	2·545	2·514
Ammonium tin	,,	$(NH_4)_2SnCl_4.3H_2O$	$(NH_4)'_3Sn'_3Cl'_3H'_4O'_2$	2·169	2·104

Table V.

Bromides.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
	a. Simple Bromides.			
Sodium bromide	$NaBr$	$Na'_4Br'_4$	3·029	3·011
Potassium "	KBr	$K'_4Br'_4$	2·674	2·672
Ammonium "	$(NH_4)Br$	$(NH_4)'_3Br'_4$	2·409	2·379
Silver "	$AgBr$	$Ag'_4Br'_4$	5·740	5·800
" "	$AgBr$	$Ag'_6Br'_4$	6·445	6·425
Strontium "	$SrBr_2$	$Sr'_4Br'_4$	4·025	3·962
Lead "	$PbBr_2$	$Pb'_6Br'_4$	6·514	6·611
Zinc "	$ZnBr_2$	$Zn'_2Br'_4$	3·600	3·643
Cadmium "	$CdBr_2$	Cd'_4Br_4	4·771	4·712
Aluminium "	Al_2Br_6	$Al'_2Br'_6$	2·571	2·540
Potassium platinobromide	K_2PtBr_6	$K'_4Pt'_2Br'_6$	4·693	4·680
Ammonium zinc bromide	$(NH_4)_2ZnBr_4$	$(NH_4)'_2Zn'_2Br'_4$	2·598	2·625
Potassium stannobromide	K_2SnBr_6	$K'_6Sn'_4Br'_4$	3·787	3·783
	b. Hydrated Bromides.			
Barium bromide	$BaBr_23H_2O$	$Ba'_4Br'_6H'_4O'_2$	3·721	3·690
" platinobromide	$BaPtBr_610H_2O$	$Ba'_4Pt'_4Br'_6H'_4O_2$	3·724	3·713
Phosphorus sulphobromide	$PSBr_3H_2O$	$P'_4S'_3Br_6H'_4O'_2$	2·771	2·793

Table VI.

Iodides.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Sodium iodide	NI	$Na'_4I'_4$	3·410	3·450
Potassium "	KI	$K'_4I'_4$	3·046	3·056
Ammonium "	$(NH_4)I$	$(NH_4)'_2I'_4$	2·417	2·498
Silver "	AgI	$Ag'_4I'_4$	5·500	5·500
Strontium "	SrI_2	$Sr'_6I'_4$	4·510	4·415
Barium "	BaI_2	$Ba'_6I'_4$	4·949	4·917
Lead "	PbI_2	$Pb'_6I'_4$	6·040	6·021
Mercurous "	HgI	$Hg'_6I'_4$	7·848	7·750
Mercuric "	HgI_2	$Hg'_6I'_4$	6·163	6·270
Arsenic "	AsI_3	$As'_2I'_6$	4·448	4·390
Antimony "	SbI_3	$Sb'_2I'_6$	5·005	5·010
Bismuth "	BiI_3	$Bi'_2I'_6$	5·555	5·652
Potassium platinoiodide	K_2PtI_6	$K'_4Pt'_2I'_6$	5·161	5·154
Ferrous iodide	FeI_24H_2O	$Fe'_3I'_4H'_4O'_2$	2·843	2·873

Table VII.

Cyanides and Cyanates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Potassium cyanide	KCy	$K'_6Cy'_2$	1·512	1·520
Silver ,,	$AgCy$	$Ag'_3(Cy)_3$	4·060	3 943
Potassium cyanate	$KCyO$	$K'_6Cy'_3O_4$	2·095	2·047
Silver ,,	$AgCyO$	$Ag'_3Cy'_3O'_4$	3·947	4·004
Sodium ferrocyanide	$Na_4Cy_6Fe12H_2O$	$Na'_3Cy'_3Fe'_2H'_4O'_2$	1·455	1·458
Potassium ,,	$K_4Cy_6Fe3H_2O$	$K'_6Cy'_3Fe'_2H'_4O'_2$	1·860	1·860
,, ferricyanide	K_3Cy_6Fe	$K'_6Cy'_3Fe'_2$	1·843	1·845
,, cobalticyanide	K_3Cy_6Co	$K'_6Cy'_3Co'_2$	1·936	1·906
Barium platinocyanide	$BaCy_4Pt$	$Ba'_2Cy'_3Pt'_2$	2·969	3·054
Potassium sulphocyanide	$KCyS$	$K'_6Cy'_3S'_6$	1·953	1·906
,, manganidcyanide	K_3Cy_6Mn	$K'_6Cy'_3Mn'_2$	1·832	1·821

Table VIII.

Sulphides.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Sodium sulphide	Na_2S	$Na'_6S'_6$	2·438	2·471
Oldhamite	CaS	$Ca'_4S'_6$	2·526	2·580
Lead monosulphide	PbS	$Pb'_4S'_6$	6·927	6·923
Mercury sulphide	HgS	$Hg'_4S'_6$	7·606	7·552
" "	HgS	$Hg'_6S'_6$	9·027	8·998
Copper "	CuS	$Cu'_3S'_6$	3·739	3·800
" "	CuS	$Cu'_6S'_6$	4·584	4·636
Zinc "	ZnS	$Zn'_3S'_8$	3·986	3·980
Cadmium "	CdS	$Cd'_3S'_8$	4·909	4·900
Tin "	SnS	$Sn'_4S'_8$	5·263	5·267
Cobalt "	CoS	$Co'_6S'_8$	5·407	5·450
Iron "	FeS	$Fe'_8S'_8$	5·008	5·035
Manganese disulphide	MnS_2	$Mn'_4S'_8$	3·424	3·463
Tin "	SnS_2	$Sn'_4S'_8$	4·494	4·415
Cobalt "	CoS_2	$Co'_6S'_8$	4·266	4·269
Arsenic "	As_2S_2	$As'_4S'_8$	3·452	3·4—3·6
Bismuth "	Bi_2S_2	$Bi'_4S'_8$	7·272	7·290
Arsenic trisulphide	As_2S_3	$As'_4S'_8$	3·324	3·400
Antimony "	Sb_2S_3	Sb'_4S_8	4·662	4·641

Table IX.

Sulphates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
a. Simple Sulphates.				
Potassium sulphate	K_2SO_4	$K'_6S'_6O_4$	2·636	2·640
Sodium ,,	Na_2SO_4	$Na'_6S'_6O'_4$	2·730	2·730
Barium ,,	$BaSO_4$	$Ba'_6S'_6O'_4$	4·568	4·590
Strontium ,,	$SrSO_4$	$Sr'_6S'_6O'_4$	3·850	3·900
Calcium ,,	$CaSO_4$	$Ca'_6S'_6O'_4$	3·068	3·100
Mercuric ,,	$HgSO_4$	$Hg'_6S'_6O'_4$	6·481	6·466
Lead ,,	$PbSO_4$	$Pb'_6S'_6O'_4$	6·269	6·257
Mercurous ,,	Hg_2SO_4	$Hg'_4S'_6O'_4$	7·630	7·560
Silver ,,	Ag_2SO_4	$Ag'_4S'_6O'_4$	5·426	5·425
Copper ,,	$CuSO_4$	$Cu'_4S'_6O'_4$	3·683	3·631
Cobalt ,,	$CoSO_4$	$Co'_4S'_6O'_4$	3·583	3·531
Ammonium ,,	$(NH_4)_2SO_4$	$(NH_4)'_3S'_6O'_4$	1·800	1·771
Zinc ,,	$ZnSO_4$	$Zn'_3S'_6O'_4$	3·331	3·400
Magnesium ,,	$MgSO_4$	$Mg'_3S'_6O'_4$	2·666	2·648
Aluminium ,,	$Al_2(SO_4)_3$	$Al'_4S'_6O'_4$	2·766	2·740
b. Double Sulphates.				
Potassium hydrogen sulphate	$KHSO_4$	$K'_6H'_2S'_6O'_4$	1·474	1·475
,, zinc ,,	$K_2Zn(SO_4)_2$	$K'_6Zn'_2S'_6O'_4$	2·780	2·816
,, copper ,,	$K_2Cu(SO_4)_2$	$K'_6Cu'_2S'_6O'_4$	2·862	2·797
,, magnesium ,,	$K_2Mg(SO_4)_2$	$K'_6Mg'_3S'_6O'_4$	2·649	2·676
,, nickel ,,	$K_2Ni(SO_4)_2$	$K'_6Ni'_3S'_6O'_4$	2·944	2·897
,, sodium ,,	$K_3Na(SO_4)_2$	$K'_6Na'_6S'_6O'_4$	2·656	2·668
,, manganese ,,	$K_2Mn(SO_4)_2$	$K'_6Mn'_6S'_6O'_4$	2·965	3·008

Table IX (*continued*).

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Ammonium aluminium sulphate	$(NH_4)Al(SO_4)_2$	$(NH_4)'_2Al'_2S'_6O'_4$	2·035	2·039
„ zinc „	$(NH_4)_2Zn(SO_4)_2$	$(NH_4)'_3Zn'_2S'_6O'_4$	2·292	2·222
Calcium sodium „	$CaNa_2(SO_4)_2$	$Ca'_4Na'_6S'_6O'_4$	2·766	2·767
„ barium „	$CaBa_3(SO_4)_4$	$Ca'_3Ba'_3S'_6O'_4$	3·330	3·2—3·4
		c. Hydrated Sulphates.		
Sodium sulphate	$Na_2SO_4.10H_2O$	$Na'_2S'_6O'_4H'_4O'_2$	1·437	1·445
Ammonium „	$(NH_4)_2SO_4.H_2O$	$(NH_4)'_3S'_6O'_4H'_4O'_2$	1·719	1·72—1·73
Copper „	$CuSO_4.5H_2O$	$Cu'_4S'_6O'_4H'_4O'_2$	2·203	2·200
Manganese „	$MnSO_4.5H_2O$	$Mn'_4S'_6O'_4H'_4O'_2$	2·060	2·087
Magnesium „	$MgSO_4.7H_2O$	$Mg'_3S'_6O'_4H'_4O'_2$	1·720	1·751
Zinc „	$ZnSO_4.7H_2O$	$Zn'_3S'_6O'_4H'_4O'_2$	1·961	1·957
Nickel „	$NiSO_4.7H_2O$	$Ni'_3S'_6O'_4H'_4O'_2$	1·955	1·931
Cobalt „	$CoSO_4.7H_2O$	$Co'_3S'_6O'_4H'_4O'_2$	1·955	1·924
Iron „	$FeSO_4.7H_2O$	$Fe'_3S'_6O'_4H'_4O'_2$	1·874	1·884
Cadmium „	$CdSO_4.H_2O$	$Cd'_2S'_6O'_4H'_4O'_2$	2·973	2·939
„ „	$Cd_3(SO_4)_3.8H_2O$	$Cd'_4S'_6O'_4H'_4O'_2$	3·000	3·050
Iron „	$Fe_2(SO_4)_3\,9H_2O$	$Fe'_2S'_6O'_4H'_4O'_2$	2·029	2·0—2·1
Aluminium „	$Al_2(SO_4)_3.18H_2O$	$Al'_2S'_6O'_4H'_4O'_2$	1·702	1·671

Table IX (*continued*).

d. Hydrated Double Sulphates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Ammonium sodium sulphate	$(NH_4)NaSO_42H_2O$	$(NH_4)'_3Na'_2S'_6O'_4H'_4O'_2$	1·621	1·630
„ magnesium „	$(NH_4)_2Mg(SO_4)_2.6H_2O$	$(NH_4)'_3Mg_2S'_6O'_4H'_4O'_3$	1·740	1·721
„ copper „	$(NH_4)_2Cu(SO_4)_2.6H_2O$	$(NH_4)'_3Cu'_2S'_6O'_4H'_4O'_2$	1·924	1·931
„ zinc „	$(NH_4)_2Zn(SO_4)_2.6H_2O$	$(NH_4)_3Zn'_2S'_6O'_4H'_4O'_2$	1·893	1·897
„ cadmium „	$(NH_4)_2Cd(SO_4)_26H_2O$	$(NH_4)_3Cd'_2S'_6O'_4H'_4O'_2$	2·043	2·073
„ nickel „	$(NH_4)_2Ni(SO_4)_26H_2O$	$(NH_4)'_3Ni'_2S'_6O'_4H'_4O'_2$	1·905	1·915
„ cobalt „	$(NH_4)_2Co(SO_4)_2.6H_2O$	$(NH_4)'_3Co'_2S'_6O'_4H'_4O'_2$	1·905	1·873
„ iron „	$(NH_4)_2Fe(SO_4)_26H_2O$	$(NH_4)'_3Fe'_2S'_6O'_4H'_4O'_2$	1·825	1·830
Potassium magnesium „	$K_2Mg(SO_4)_26H_2O$	$K'_6Mg'_3S'_6O'_4H'_4O'_2$	2·061	2·076
„ copper „	$K_2Cu(SO_4)_26H_2O$	$K'_6Cu'_3S'_6O'_4H'_4O'_2$	2·255	2·244
„ zinc „	$K_2Zn(SO_4)_26H_2O$	$K'_6Zn'_3S'_6O'_4H'_4O'_2$	2·234	2·240
„ cobalt „	$K_2Co(SO_4)_26H_2O$	$K'_6Co'_2S'_6O'_4H'_4O'_2$	2·179	2·154
„ cadmium „	$K_2Cd(SO_4)_26H_2O$	$K'_6Cd'_4S'_6O'_4H'_4O'_2$	2·462	2·438
„ iron „	$K_2Fe(SO_4)_26H_2O$	$K'_6Fe'_4S'_6O'_4H'_4O'_2$	2·205	2·202
Sodium alum	$AlNa(SO_4)_212H_2O$	$Al'_2Na'_2S'_6O'_4H'_4O'_2$	1·637	1·641
Ammonium „	$Al(NH)_4(SO_4)12H_2O$	$Al'_2(NH_4)_3S_6O'_4H'_4O'_2$	1·651	1·653
Potassium „	$AlK(SO_4)_212H_2O$	$Al'_2K'_6S'_6O'_4H'_4O'_2$	1·751	1·753
„ chrome alum	$CrK(SO_4)_212H_2O$	$Cr'_3K'_6S'_6O'_4H'_4O'_2$	1·850	1·848
Ammonium „ „	$Cr(NH_4)(SO_4)_212H_2O$	$Cr'_3(NH_4)'_3S'_6O'_4H'_4O'_2$	1·748	1·738
„ iron „	$Fe(NH_4)(SO_4)_212H_2O$	$Fe'_2(NH_4)'_3S'_6O'_4H'_4O'_2$	1·714	1·712
Magnesium iron sulphate	$MgFe(SO_4)_214H_2O$	$Mg'_2Fe'_2S'_6O'_4H'_4O'_2$	1·730	1·733
„ copper „	$MgCu(SO_4)_2.14H_2O$	$Mg'_2Cu'_3S'_6O'_4H'_4O'_2$	1·825	1·813
„ zinc „	$MgZn(SO_4)_214H_2O$	$Mg'_2Zn'_3S'_6O'_2H'_4O'_2$	1·814	1·817
„ cadmium „	$MgCd(SO_4)_214H_2O$	$Mg'_2Cd'_4S'_6O'_4H'_4O'_2$	1·970	1·983

Table X.

Nitrates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
a. Simple Nitrates.				
Sodium nitrate	$NaNO_3$	$Na'_6N'_2O'_3$	2·125	2·180
Potassium ,,	KNO_3	$K'_6N'_2O'_3$	2·148	2·143
Ammonium ,,	$(NH_4)NO_3$	$(NH_4)'_3N'_2O'_3$	1·579	1·579
,, ,,	,,	$(NH_4)'_4N'_2O'_3$	1·739	1·737
Silver ,,	$AgNO_3$	$Ag'_6N'_2O'_3$	4·340	4·336
Calcium ,,	$Ca(NO_3)_2$	$Ca'_6N'_2O'_3$	2·267	2·240
Strontium ,,	$Sr(NO_3)_2$	$Sr'_6N'_2O'_3$	2·807	2·857
Barium ,,	$Ba(NO_3)_2$	$Ba'_6N'_2O'_3$	3·289	3·284
Lead ,,	$Pb(NO_3)_2$	$Pb'_6N'_2O'_3$	4·336	4·340
b. Hydrated Nitrates.				
Calcium nitrate	$CaN_2O_6.4H_2O$	$Ca'_6N'_2O'_3H'_4O'_2$	1·843	1·78—1·90
Strontium ,,	$SrN_2O_6.5H_2O$	$Sr'_6N'_2O'_3H'_4O'_2$	2·069	2·113
Manganese ,,	$MnN_2O_6.6H_2O$	$Mn'_4N'_2O'_3H'_4O'_2$	1·836	1·819
Cobalt ,,	$CoN_2O_6.6H_2O$	$Co'_3N'_2O'_3H'_4O'_2$	1·845	1·830
Iron ,,	$Fe_2(NH_3)_6.18H_2O$	$Fe'_3N'_2O'_3H'_4O'_2$	1·692	1·683
Bismuth ,,	$BiN_3O_9.5H_2O$	$Bi'_6N'_2O'_3H'_4O'_2$	2·688	2·736

Table XI.

Chromates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
a. Simple Chromates.				
Potassium chromate	K_2CrO_4	$K'_6Cr'_2O'_4$	2·682	2·682
Silver „	$AgCrO_4$	$Ag'_6Cr'_2O'_4$	5·850	5·770
Barium „	$BaCrO_4$	$Ba'_4Cr'_2O'_4$	3·870	3·900
Lead „	$PbCrO_4$	$Pb'_6Cr'_2O'_4$	5·900	5·950
Potassium dichromate	$K_2Cr_2O_7$	$K'_6Cr'_2O'_4$	2·682	2·680
Ammonium „	$(NH_4)_2Cr_2O_7$	$(NH_4)'_4Cr'_2O'_4$	2·343	2·367
Potassium trichromate	$K_2Cr_3O_{10}$	$K'_6Cr'_2O'_4$	2·681	2·655
Phœnicochroit	$Pb_3Cr_2O_9$	$Pb'_6Cr'_2O'_4$	5·980	6·000
b. Hydrated Chromates.				
Copper chromate	$CuCrO_45H_2O$	$Cu'_4Cr'_2O'_4H'_4O'_2$	2·254	2·262
Zinc „	$ZnCrO_47H_2O$	$Zn'_4Cr'_2O'_4H'_4O'_2$	2·053	2·096
Magnesium „	$MgCrO_47H_2O$	$Mg'_2Cr'_2O'_4H'_4O'_2$	1·730	1·750

Table XII.

Phosphates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
a. Simple Phosphates.				
Trisilver phosphate	Ag_3PO_4	$Ag'_6P'_6O'_4$	7·163	7·300
Lead ,,	$Pb_3(PO_4)_2$	$Pb'_6P'_6O'_4$	7·306	7·208
Silver pyrophosphate	$Ag_4P_2O_7$	$Ag'_4P'_6O'_4$	5·410	5·306
Ammonium dihydrogen phosphate	$(NH_4)H_2PO_4$	$(NH_4)'_3H'_2P'_6O'_4$	1·805	1·758
Diammonium hydrogen ,,	$(NH_4)_2HPO_4$	$(NH_4)'_3H'_2P'_6O'_4$	1·685	1·678
b. Hydrated Orthophosphates.				
Trisodium phosphate	$Na_3PO_4.12H_2O$	$Na'_6P'_6O'_4H'_4O'_2$	1·659	1·622
Sodium pyrophosphate	$Na_4P_2O_7 10H_2O$	$Na'_6P'_6O'_4H'_4O_2$	1·850	1·836
Disodium hydrogen phosphate	$Na_2HPO_4 12H_2O$	$Na'_4H'_4P'_6O'_4H'_4O'_2$	1·549	1·550
Dihydrogen sodium ,,	$NaH_2PO_4H_2O$	$Na'_4H'_4P'_6O'_4H'_4O'_2$	2·059	2·040
Triple phosphate No. I	$(NH_4)NaHPO_4 4HO$	$(NH_4)'_2Na'_4H'_4P'_6O'_4H'_4O'_2$	1·549	1·554
,, ,, No. II	$KNaHPO_4.7H_2O$	$K'_4Na'_4H'_4P'_6O'_4H'_4O'_2$	1·657	1·671
Struvite	$(NH_4)MgPO_4.6HO_2$	$(NH_4)'_3Mg'_3P'_6O'_4H'_4O'_2$	1·650	1·650
Berlinite	$Al_4(PO_4)_4.H_2O$	$Al'_4P'_6O'_4H'_4O'_2$	2·620	2·640

Table XIII.

Arsenites and Arsenates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
a. Simple Arsenites and Arsenates.				
Lead arsenite	$Pb(AsO_2)_2$	$Pb'_6As'_4O'_4$	5·985	5·850
Native nickel arsenate	$Ni_3(AsO_4)_2$	$Ni'_6As'_4O'_4$	4·919	4·982
Potassium „	KH_2AsO_4	$K'_4H'_4As'_4O'_4$	2·748	2·638
Ammonium „	$(NH_4)H_2AsO_4$	$(NH_4)'_2H'_4As'_4O_4$	2·240	2·249
b. Hydrated Arsenates.				
Sodium dihydrogen arsenate	$NaH_2AsO_4H_2O$	$Na'_4H'_2As'_4O'_4H'_4O'_2$	2·494	2·535
Disodium hydrogen „	$Na_2HAsO_4.7H_2O$	$Na'_4H'_2As'_4O'_4H'_4O'_2$	1·891	1·871
„ „ „	$Na_2HAsO_4.12H_2O$	$Na'_4H'_2As'_4O'_4H'_4O'_2$	1·711	1·736
Trisodium arsenate	$Na_3AsO_4.12H_2O$	$Na'_6As'_4O'_4H'_4O'_2$	1·835	1·804
Triple arsenate No. I	$(NH_4)NaHAsO_4.4H_2O$	$(NH_4)'_2Na'_4H'_4As'_4O'_4H'_4O'_2$	1·846	1·838
„ „ No. II	$KNaHAsO_4.7H_2O$	$K'_4Na'_4H'_4As'_4O'_4H'_4O'_2$	1·890	1·884
Hoernesite	$Mg_3(AsO_4)_2.8H_2O$	$Mg'_6As'_4O'_4H'_4O'_2$	2·427	2·474
Erythrite	$Co_3(AsO_4)_8.8H_2O$	$Co'_6As'_4O'_4H'_4O'_2$	2·924	2·948
Scorodite	$Fe_2(AsO_4)_2.4H_2O$	$Fe'_6As'_4O'_4H'_4O'_2$	3·115	3·110

Table XIV.

Borates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Sodium diborate	$Na_2B_4O_7$	$Na'_4B'_4O'_4$	2·322	2·367
,, ,,	$Na_2B_4O_7.5H_2O$	$Na'_4B'_4O'_4H'_4O'_2$	1·859	1·815
,, ,,	$Na_2B_4O_7 10H_2O$	$Na'_4B'_4O'_4H'_4O'_2$	1·683	1·692
Lead ,, borate	PbB_2O_4	$Pb'_4B'_4O'_4$	5·580	5·598
,, hydrogen ,,	$PbHB_3O_6$	$Pb'_6H'_4B'_4O'_4$	5·208	5·235
Magnesium ,,	$Mg_3B_2O_6$	$Mg'_4B'_4O'_4$	2·988	2·987

Table XV.

Chlorates and Bromates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Sodium chlorate	$NaClO_6$	$Na'_4Cl'_3O'_4$	2·535	2·467
Potassium ,,	$KClO_3$	$K'_4Cl'_3O'_4$	2·333	2·326
Lead ,,	$Pb(Cl_2O_3)_2.H_2O$	$Pb'_6Cl'_3O'_4H'_4O'_2$	3·981	3·989
Mercury ,,	$Hg_2Cl_2O_7H_2O$	$Hg'_3Cl'_3O'_4H'_4O'_2$	5·107	5·151
Sodium bromate	$NaBrO_3$	$Na'_6Br'_4O'_4$	3·355	3·339
Potassium ,,	$KBrO_3$	$K'_6Br'_4O'_4$	3·211	3·218
Magnesium ,,	$Mg(BrO_3)_2 6H_2O$	$Mg'_2Br'_4O'_4H'_4O'_2$	2·262	2·289

Table XVI.

Silicates and Titanates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
	a. Simple Silicates and Titanates.			
Wollastonite	$CaSiO_3$	$Ca'_3Si'_4O'_8$	2·800	2·805
Rhodonite	$MnSiO_3$	$Mn'_4Si'_4O'_8$	3·678	3·630
Grünerite	$FeSiO_3$	$Fe'_4Si'_4O'_8$	3·706	3·713
Enstatite	$MgSiO_3$	$Mg'_4Si'_4O'_8$	3·165	3·130
Tephroite	Mn_2SiO_4	$Mn'_4Si'_4O'_8$	4·132	4·120
Fayalite	Fe_2SiO_4	$Fe'_4Si'_4O'_8$	4·173	4·138
Willemite	$Zn_2Si_2O_4$	$Zn'_4Si'_4O'_8$	3·922	3·935
Olivine	Mg_2SiO_4	$Mg'_4Si'_4O'_8$	3·426	3·440
Andalusite	Al_2SiO_5	$Al'_3Si'_4O'_8$	3·185	3·154
„ cyanite	Al_2SiO_5	$Al'_4Si'_4O'_8$	3·554	3·480—3·680
Zircon	$ZrSiO_4$	$Zr'_4Si'_4O'_8$	4·136	
Calcium titanate	$CaTiO_3$	$Ca'_4Ti'_4O'_8$	4·000	4·000
Magnesium „	$MgTiO_3$	$Mg'_3Ti'_4O'_8$	3·935	3·910
	b. Double Silicates.			
Felspar	$KAlSi_3O_8$	$K'_4Al'_2Si'_4O'_8$	2·522	2·53—2·59
Albite	$NaAlSi_3O_8$	$Na'_4Al'_2Si'_4O'_8$	2·627	2·54—2·64
Batrachite	$CaMg(SiO_3)_2$	$Ca'_4Mg'_3Si'_4O'_8$	3·034	3·033
Monticellite	$CaMg(SiO_3)_2$	$Ca'_6Mg'_3Si'_4O'_8$	3·222	3·245—3·275
Leucite	$KAl(SiO_3)_2$	$K'_4Al'_2Si'_4O'_8$	3·482	2·45—2·5

Table XVII.

Carbonates.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
	a. Simple Carbonates.			
Barium carbonate	$BaCO_3$	$Ba'_4C'_4O'_4$	4·283	4·301
Strontium „	$SrCO_3$	$Sr'_4C'_4O'_4$	3·642	3·626
Calcium „	$CaCO_3$	$Ca'_4C'_4O'_4$	2·817	2·815
Lead „	$PbCO_3$	$Pb'_4C'_4O'_4$	6·433	6·430
Silver „	Ag_2CO_3	$Ag'_4C'_4O'_4$	6·202	6·080
	b. Simple Carbonates.			
Sodium carbonate	Na_2CO_3	$Na'_4C'_8O'_4$	2·469	2·466
Potassium „	K_2CO_3	$K'_4C'_8O'_4$	2·156	2·103—2·267
Ferric „	$FeCO_3$	$Fe'_4C'_8O'_4$	3·891	3·872
Zinc „	$ZnCO_3$	$Zn'_4C'_8O'_4$	4·424	4·420
Magnesium „	$MgCO_3$	$Mg'_3C'_8O'_4$	3·000	3·017
	c. Double Carbonates.			
Barytocalcite	$CaBa(CO_3)_2$	$Ca'_4Ba'_4C'_4O'_4$	3.666	3·660
Manganocalcite	$CaMn_2(CO_3)_3$	$Ca'_4Mn'_3C'_4O'_4$	3·000	3·037
Dolomite	$CaMg(CO_3)_2$	$Ca'_4Mg'_4C'_4O'_4$	2·819	2·845
Pistomesite	$FeMg(CO_3)_2$	$Fe'_4Mg'_3C'_8O'_4$	3·463	3·427
Mesitite	$FeMg_2(CO_3)_3$	$Fe'_4Mg'_3C'_8O'_4$	3·311	3·349
	d. Hydrated Double Carbonates			
Sodium carbonate	$Na_2CO_3H_2O$	$Na'_2C'_8O'_4H'_4O'_2$	1·531	1·5—1·6
Calcium „	$CaCO_3.5H_2O$	$Ca'_4C'_4O'_4H'_4O'_2$	1·800	1·783
Sodium potassium „	$KNaCO_312H_2O$	$K'_6Na'_6C'_8O'_4H'_4C'_2$	1·609	1·608
Gay-lussite	$CaNa_2(CO_3)_25H_2O$	$Ca'_4Na'_4C'_4O'_4H'_4O'_2$	1·941	1·950
Hydrodolomite	$CaMg_2(CO_3)_3H_2O$	$Ca'_3Mg'_4C'_4O'_4H'_4O'_2$	2·535	2·495

Table XVIII.

Metallic Salts of Organic Acids.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
a. Oxalates.				
Potassium oxalate	$K_2C_2O_4H_2O$	$K'_4C'_8O'_4H'_4O'_2$	2·114	2·104
Ammonium "	$(NH_4)_2C_2O_4H_2O$	$(NH_2)'_2C'_8O'_4H'_4O'_2$	1·450	1·461
Hydrogen sodium "	$NaHC_2O_4H_2O$	$Na'_4H'_4C'_8O'_4H'_4O'_2$	2·321	2·315
Potassium quadroxalate	$KH_3(CO_4)_22H_2O$	$K'_2H'_2C'_8O'_4H'_4O'_2$	1·802	1·817
Hydrogen ammonium oxalate	$(NH_4)HC_2O_4.H_2O$	$(NH_4)'_2H'_4C'_4O'_4H'_4O'_2$	1·562	1·563
" potassium "	KHC_2O_4	$K'_4H'_2C'_4O'_4$	2·080	2·088
Potassium copper "	$K_2CuC_4O_8.2H_2O$	$K'_4Cu'_3C'_4O'_4H'_4O'_2$	2·285	2·288
Ammonium " "	$(NH_4)_2Cu(C_2O_4)_22H_2O$	$(NH_4)'_2Cu'_4C'_4O'_4H'_4O'_2$	1·908	1·923
Whewellite	CaC_2O_4	$Ca'_4C'_4O'_4$	2·666	2·50—2·75
b. Acetates.				
Sodium acetate	$NaC_2H_3O_2$	$Na'_4C'_4H'_3O'_2$	1·464	1·421
" "	$NaC_2H_3O_26H_2O$	$Na'_6C'_4H'_3O'_2H'_4O'_2$	1·400	1·400
Silver "	$AgC_2H_3O_2$	$Ag'_4C'_4H'_3O'_2$	3·050	3·128
Lead "	$Pb(C_2H_3O_2)_2.3H_2O$	$Pb'_3C'_4H'_3O'_2H'_4O'_2$	2·471	2·496
Barium "	$Ba(C_2H_3O_2)_2H_2O$	$Ba'_4C'_4H'_3O'_2H'_4O'_2$	2·184	2·190
Zinc "	$Zn(C_2H_3O_2)_23H_2O$	$Zn'_4C'_4H'_3O'_2H'_4O'_2$	1·701	1·717
c. Tartrates.				
Sodium tartrate	$Na_2C_4H_4O_64H_2O$	$Na'_4C'_4H'_4O'_4H'_4O'_2$	1·773	1·794
Potassium "	$K_2C_4H_4O_6$	$K'_4C'_4H'_4O'_4$	1·965	1·975
Hydrogen potassium "	$KHC_4H_4O_6$	$K'_4H'_2C'_4H'_4O'_4$	1·948	1·943
" ammonium "	$(NH_4)HC_4H_4O_6$	$(NH_4)'_2H'_4C'_4H'_4O'_4$	1·670	1·680
Sodium potassium "	$NaKC_4H_4O_64H_2O$	$Na'_4K'_4C'_4H'_4O'_4H'_4O'_2$	1·757	1·767
" ammonium "	$(NH_4)NaC_4H_4O_64H_2O$	$(NH_4)'_2Na'_4C'_4H'_4O'_4H'_4O'_2$	1·572	1·576

Table XIX.

Organic Compounds.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Naphthalene	$C_{10}H_8$	$C'_4H'_2$	1·143	1·153
Anthracene	$C_{14}H_{10}$	$C'_4H'_2$	1·171	1·147
Paraffin	$X(CH_2)$	$X(C'_4H'_2)$	0·875	0·870
Caoutchouc	$X(C_5H_8)$	$X(C'_4H'_2)$	0·944	0·92—·906
Succinic acid	$C_4H_6O_4$	$C'_4H'_2O'_4$	1·552	1·552
Essence of aniseed	$C_{10}H_{12}O$	$C'_4H'_2O'_2$	1·072	1·073
Camphor	$C_{10}H_{16}O$	$C'_4H'_2O'_3$	0·987	0·996
Starch	$X(C_6H_{10}O_5)$	$X(C'_4H'_3O'_3)$	1·500	1·500
Sugar	$C_{12}H_{22}O_{11}$	$C'_4H'_4O'_3$	1·603	1·606
Perchlorinated ether	$C_4Cl_{10}O$	$C'_2Cl'_3O'_4$	1·913	1·900
Trichloracetic acid	$HC_2Cl_3O_2$	$H'_2C'_2Cl'_3O'_2$	1·618	1·617
Urea	CH_4N_2O	$C'_4H'_2N'_4O'_2$	1·304	1·300

Table XX.

Ammonio Compounds.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Cadmium ammonio chloride	$CdCl_2 2NH_3$	$Cd'_3Cl'_3N'_2H'_4$	2·605	2·632
Pupureo cobalt chloride	$Co_2Cl_6 10NH_3$	$CO'_6Cl'_3N'_2H'_4$	1·800	1·802
Luteo ,, ,,	$Co_2Cl_6 12NH_3$	$CO'_6Cl'_3N'_2H'_4$	1·706	1·701
Copper ammonio ,, 1st	$CuCl_2.2NH_3$	$Cu'_3Cl'_3N'_2H'_4$	2·225	2·194
,, ,, ,, 2nd	$CuCl_2 4NH_3H_2O$	$Cu'_2Cl'_3N'_2H'_4H'_4O'_2$	1·689	1·672
Silver ,, sulphate	$Ag_2SO_4 4NH_3$	$Ag'_4S'_6O'_4N'_2H'_4$	2·933	2·918
,, ,, chromate	$Ag_2CrO_4 4NH_3$	$Ag'_6Cr'_2O'_4N'_2H'_4$	3·108	3·063
Copper ,, sulphate	$CuSO_4 2NH_3$	$Cu'_4S'_6O'_4N'_2H'_4$	2·441	2·476
,, ,, ,,	$CuSO_4 4NH_3H_2O$	$Cu'_2S'_6O'_4N'_2H'_4H'_4O'_2$	1·798	1·790
,, ,, ,,	$CuSO_4 2NH_3 3H_2O$	$Cu'_2S'_6O'_4N'_2H'_4H'_4O'_2$	1·926	1·950

Table XXI.

Miscellaneous Compounds.

Substance.	Molecular weight.	Molecular volume.	Calculated sp. gr.	Observed sp. gr.
Cyanogen iodide	CyI	$Cy'_3I'_2$	1·850	1·850
Chromic chloride	Cr_2Cl_6	$Cr'_6Cl'_3$	3·028	3·030
„ chromate	Cr_5O_9	$Cr'_4O'_4$	4·014	4·000
Nitrogen sulphide	NS	$N'_4S'_5$	2·090	2·116
Sodium hydrate	NaHO	$Na'_4H'_4O'_4$	2·105	2·130
Zinc „	ZnH_2O_2	$Zn'_2H'_4O'_4$	3·046	3·053
Boric acid	H_3BO_3	$H'_3B'_2O'_3$	1·476	1·479
Sodium sulphite	$Na_2SO_310H_2O$	$Na'_4S'_6O'_4H'_4O'_2$	1·569	1·561
„ hyposulphite	$Na_2SO_35H_2O$	$Na'_4S'_6O'_4H'_4O'_2$	1·758	1·736
Potassium disulphite	$K_2S_2O_6$	$K'_4S'_6O'_4$	2·224	2·277
Sodium „	$Na_2S_2O_62H_2O$	$Na'_4S'_6O'_4H'_4O'_2$	2·122	2·189
Calcium „	$CaS_2O_64H_2O$	$Ca'_6S'_6O'_4H'_4O'_2$	2·153	2·180
Turpeth mineral	Hg_3SO_6	$Hg'_4S'_6O'_4$	8·134	8·319
Romeite	$Ca_3Sb_4O_{11}$	$Ca'_4Sb'_4O'_4$	4·737	4·714
Sodium antimonite	$NaSbO_23H_2O$	$Na'_4Sb'_4O'_4H'_4O'_2$	2·808	2·864
Silicohydric chloride	$Si_3H_4Cl_{10}$	$Si'_2H'_2Cl_3$	1·640	1·650

"On a New Form of Febrile Disease associated with the presence of an Organism distributed with Milk from the Oldmill Reformatory School, Aberdeen." By J. COSSAR EWART, M.D., Professor of Natural History in the University of Aberdeen. Communicated by Professor BURDON SANDERSON, F.R.S. Received and Read June 16, 1881.

History of the Outbreak.—On the 1st of April one of my servants was prostrated with a peculiar fever, which at first seemed to be a severe attack of influenza. No special treatment was indicated, and it did not appear necessary to call in a medical attendant. When, however, on the following day a child of nine months was attacked in the same way, I felt alarmed, and at once proceeded to inquire as to the nature of the disease, and as to its probable origin.

I soon heard of about twenty other cases, all suffering in the same way, and all living under similar conditions and in the same neighbourhood, using the same water, and, without exception, all obtaining their milk from the same dairy.

Knowing how often milk has been a means of disseminating fever organisms, I secured the milk on delivery the next morning (April 3), that I might examine it microscopically, and subject it to other tests, should such seem necessary.

Further inquiry carried on by Dr. Beveridge, Chairman of the Aberdeen Town Council Health Committee, showed that the same fever followed fast in the track of the milk-cart from the Oldmill Reformatory School. Out of 110 families supplied with Oldmill milk, at least 89 suffered from this peculiar fever; there being 220 cases in 66 of these families; in all, about 320 individuals are known to have suffered.

Careful inquiry failed to discover a single case where the Oldmill milk had not been in use in one form or another.

The fever seems to have made its appearance on the 25th of March, but to have made but slow progress until the 1st of April, when 13 cases were reported. On the 2nd of April 22 cases were reported; there were 22 more on the 3rd of April, 31 on the 4th of April, 27 on the 5th April, 50 on the 6th April, and 16 on the 7th April. By the 6th April many families had ceased to use the Oldmill milk, and on the 7th April the Directors of the Reformatory stopped the supply. On the day the supply was stopped, 16 fresh cases were reported. On the 8th April 6 were reported, another was reported on the 9th April, and the only other known case was reported on the 13th April.

From the evidence given before the Commissioners, appointed under the Public Health (Scotland) Act to investigate the nature and cause of the epidemic, it appeared that some of the families which escaped had been supplied with milk of a very superior quality; whereas many of those who suffered from the disease deponed that the milk delivered to them looked and tasted as if it had either been adulterated with water or with skimmed milk. In other families which escaped it was only used for cooking purposes, or the chief supply of milk was obtained from other sources, or only a very small quantity of cream was used. In many cases the individual members of affected families who escaped were in the habit of using the milk only after it had been cooked or boiled. In my own household of eight, two escaped who were in the habit of using boiled milk only.

Symptoms.—In the cases which came under my observation, I noticed the following symptoms:—

1. A well-marked rigor, with a feeling of chilliness, lasting from ten to fifteen hours; headache, pain in the back and limbs, and often nausea and vomiting.

2. Sudden rise of temperature varying from 103° to 104°, and even reaching sometimes 105° F.

3. Hot dry skin, and a pulse of 120 to 125.

4. A feeling of uneasiness in the neck, accompanied in some cases with congestion of the fauces and tonsils.

5. Enlargement of the cervical glands lying between the angle of the jaw and the anterior border of the sterno-mastoid muscle—the glands of one side were usually more affected than those on the other.

6. Complete remission of the febrile symptoms for twenty-four to thirty-six hours, but marked prostration during the remission.

7. This period was followed by a return, in nearly all the cases, of the high temperature, which lasted from twenty to thirty-six hours. In many cases, after the second remission, there was a third relapse, and in not a few cases a fourth relapse. In those cases in which a relapse took place, the glands least affected during the first attack were most affected during the second attack.

8. In all cases there was a very slow recovery from the prostration. Although the outbreak began on the 25th of March, some of those attacked are still (13th June) suffering from sequelæ, to be afterwards referred to.

Perhaps the most characteristic and constant feature of the disease was the swelling of the cervical glands at the angle of the jaw.

Further, there was an indurated condition apparently of the fibrous structures in front of the sterno-mastoid, extending as far as the clavicle, and sometimes upwards, so as to appear to involve the parotid gland. In most cases this induration slowly disappeared, but in at least three cases it has ended in suppuration.

In many cases, after convalescence had fairly set in, there were frequent relapses characterised chiefly by great prostration, which lasted from two to three days, and not a few suffered from attacks of a condition closely resembling muscular and articular rheumatism.

There were three fatal cases. It is to be remarked, however, that these patients were elderly people (over seventy years of age), and that they seem to have succumbed to the prostration produced by the disease.

Arrangements at the Dairy.

The microscopic examination of the milk of the 3rd April showed that it contained numerous organisms; hence I visited the dairy in order if possible to learn how these organisms could have found their way into the milk. The cows, as far as I could judge, were perfectly healthy, their udders clean, and no abrasion about the muzzles such as exist in cattle affected with foot-and-mouth disease. The food at the time of my visit consisted of hay, straw, grains ("draff"), bran, oil-cake, and turnips. The drinking water came from a cistern which was placed in a corner of the large open byre immediately over the heads of several cows.

The cistern was made of concrete and only loosely covered with a thin wooden lid, consisting of at least two separate pieces, which had at one time been connected by a leather hinge. This cistern was capable of containing about 300 gallons of water. On further inquiry I found that all the water used about the dairy for washing the cans, for dissolving the nitrate of potash added to the milk, came from this cistern, and that somewhere about 600 gallons of water were used daily; hence the level of the water would be constantly falling and rising in the cistern, and the air from the byre constantly passing in and out of the cistern. Two days previous to my visit the use of the cistern had been discontinued. On going into the cistern I found a small quantity of water at the bottom, and secured a sample of it for future examination. I noticed also that fungi flourished on the inside of the wooden lid, and that some parts of the sides of the cistern, notwithstanding the scrubbing the cistern had just undergone, were coated with a thin layer of what might at one time have been a scum on the surface—a scum formed during the winter or during February, when the water supply was cut off for several days.

The hay used for feeding the cows was cut into small pieces and steamed in a large tank before it was used. After being steamed it was allowed to cool and then conveyed to the byre in wooden troughs. Enough was steamed at a time to last for two or three days. The day after the steaming the bottom of the tank was found to be filled with a strong infusion of hay. Some of this infusion I carried off for examination. On the same level as this tank, and only separated from it by a narrow partition, was a tank of about the same size, which was used for keeping the grains used. The manager informed me that he considered the grains of the previous week to be of very inferior quality. The turnips, considering the season of the year and the severity of the winter, looked well.

The general impression produced on me by my visit was that the water was at fault. This impression was strengthened by the following circumstances:—(1.) The dairymaid had recently complained that the water had a very bad smell; (2.) The manager gave it as his decided opinion that the water had been the cause of the epidemic; and (3.) On chemical analysis by Mr. Jamieson, the city analyst, it was found to contain a large quantity of "albuminoid ammonia." Mr. Jamieson reported that "the contamination which the water as supplied to Aberdeen has suffered takes it from the 'excellent' into the 'dangerous' class; while the harmless ingredients remain at nearly the same point, the injurious ingredients have been trebled."

The following is the result of the chemical analysis by Mr. Jamieson:—

	Grains per gallon.	Parts per million.
Total solids containing........	4·7	67·143
Chlorine....................	..	10·143
Free ammonia...............	..	0·029
"Albuminoid ammonia"......	..	0·212
Nitric anhydride.............	..	3·857
Sulphate of lime and carbonaceous matter..............	..	52·902
		67·143

Results of Microscopic Investigations.

1. *The Milk.*—Professor Brazier reported of a sample of milk sent him for examination on 5th April as follows:—

"The sample of milk, chemically considered, appeared to be one of fair average quality. The proportion of water and of saline matter was by no means abnormal and gave no reason for suspecting any admixture or adulteration therewith. The only thing that attracted my attention was the somewhat rank odour possessed by the milk, which manifested itself the more by allowing the milk to rest for a time in a vessel half-full and loosely covered."

On microscopic examination of the milk delivered on Sunday, 3rd April, I found—(1.) Numerous micrococci. Some of them were free and not readily recognised, but many were united so as to form the characteristic "micrococci chains," while others were grouped together, forming zooglœa.

(2.) Numerous spores of fungi and cells of the yeast plant.

(3.) Spores similar to the spores of *B. anthracis.*

On examining milk purchased from another dairy and milk sent from the Oldmill Dairy on Wednesday, 6th April, after the alarm had been raised about the milk, and the use of the cistern discontinued, it was impossible to detect with the microscope either micrococci or the spores of bacteria or fungi.

2. Water from the cistern in the byre contained numerous yeast cells, fungus spores, and a few micrococci, but I was unable to detect any spores of bacteria.

3. The infusion from the hay tank teemed with bacilli, ordinary bacteria, micrococci, and "micrococci chains," and there were also numerous filaments in process of development, and numerous yeast-cells.

4. Hay infusion recently made contained bacilli, bacteria, and micrococci, but only a small number of yeast cells.

5. The grains contained bacilli, bacteria, and micrococci, but especially the cells of *Torula.*

6. The soft centre of a turnip presented a few ordinary bacteria and a few bacilli.

Results of Cultivations.

1. *3rd of April. Oldmill Milk.*—On cultivating the milk referred to above in aqueous humour the micrococci became very active, and many of them assumed a dumb-bell shape, which indicated that they were in process of multiplication. The spores previously observed began to germinate and in a few hours fully developed rods made their appearance. The bacilli, as is usually the case with *B. anthracis*, remained motionless, any apparent oscillation resulting from the active movements of the micrococci around them. In from fourteen to twenty hours the bacilli began to lengthen, but, probably owing to the abundant presence of micrococci, their development was arrested in this and in all subsequent cultivations of the milk before they reached the spore-bearing stage.

On cultivating in the same way—(1) Milk sent from Oldmill on the 6th April; (2) Milk collected directly from two Oldmill cows in clean stoppered bottles on the 27th April; and (3) Milk from another dairy (Cowie), no bacilli made their appearance; there were, however, in (1) and (3) a few ordinary bacteria (*B. termo?*) and numerous micrococci.

On cultivating the Oldmill hay infusion, the fresh hay infusion, the grains and turnips, there was a considerable increase in the number of bacilli, bacteria, and micrococci, and in the case of the grains and Oldmill hay infusion a great increase of the *Torula* cells, and many of the fungus spores germinated into branching filaments.

PATHOLOGICAL RESULTS.

First Series of Experiments.

a. Inoculated rat with distilled water. No result.

b. Inoculated rat with milk from Cowie Dairy. No result.

c. Inoculated rat with cultivated milk from Cowie Dairy. No result.

d. Inoculated rat with milk from Oldmill Dairy, taken on 27th April. No result.

e. Inoculated rat with milk, 27th April, after cultivation. No result.

f. Inoculated rat with Oldmill milk of 3rd April, containing spores, micrococci, and Torulæ.

g. Inoculated rat with milk of 3rd April, after cultivation, containing bacilli, short filaments, micrococci, and Torulæ.

Death followed in both cases from fifteen to twenty hours after inoculation. After several failures, I succeeded in obtaining a cultivation from near the seat of inoculation, containing bacilli only, or at least very few micrococci. In due time the bacilli developed into spore-bearing filaments.

Inoculation with the spores from this cultivation produced the same result as the milk and the milk cultivations *f* and *g*, but the spores were slower in their action, and now ([illegible]th June) the fatal result does not follow until four days after inoculation with cultivated spores.

Second Series of Experiments.

Other rats were inoculated with the fresh hay infusion, with the turnip infusion, with an infusion of grains, with the Oldmill hay infusion, and with distilled water. No result followed except in one instance. A rat, twenty-five hours after inoculation with the Oldmill hay infusion, was found dead. I have not had time to complete this series of experiments.

Third Series of Experiments.

From one of the cases in which the induration in the neck ended in suppuration,[*] I obtained a small quantity of pus, antiseptically collected and sealed in vaccine tubes. On examination the pus was seen to contain a number of bacilli and spores, which exactly resembled the spores previously found in the 3rd April Oldmill milk. On placing a very small drop of this pus in a cell and introducing the cell into a warm chamber, the spores germinated and some of the rods lengthened into filaments, but the filaments have not yet (13th June) developed spores.

Pus collected in the same way, but apparently containing neither bacilli nor their spores, has not, under cultivation, undergone any change.

With an absolutely new syringe, I inoculated—(1) a rat with a very small quantity of the pus containing the bacilli and spores; the pus was taken from the capillary vaccine tube, and dissolved in a few drops of distilled water; (2) another rat was inoculated with the pus in which there were no bacilli; and (3) a rat was inoculated with distilled water.

The rat inoculated with the pus containing the bacilli was found dead the third day after the inoculation, the other rats are still alive.

In the tissues of the rat which died are numerous bacilli, which, on cultivation, lengthen into spore-bearing filaments.

I have not been able to detect any special lesions in the rats which succumbed to the several inoculations. The right side of the heart is engorged, and the subcutaneous tissue, especially around the seat of inoculation and in the cervical region, is infiltrated with colourless blood-corpuscles, but the alimentary tract and spleen retain their normal appearances.

My investigations have led me to conclude—(1.) That this disease (which is considered by all the medical men in the district to be a new disease) has been set up by an organism morphologically not unlike *B. anthracis* and having the same life history.

(2.) That the organism was introduced into the milk after it left the udder of the cow.

(3.) That the conditions in and around the dairy were such as would give special organisms, conveyed to the dairy in the grains or hay, every opportunity of developing and finding their way into the water cisterns. In fact, the conditions were such that an innocuous organism might easily have assumed noxious properties.

Whether the washing of the milk cans with the infected water is sufficient to account for the outbreak, I am not prepared to say; but judging from the immense number of organisms in the milk of the 3rd April, compared with the milk of 6th April, in which there were few if any organisms, I am inclined to believe that there must have been other channels through which the milk was infected.

Note.—I am indebted to Professor Stephenson for the opportunity of experimenting with pus from one of the affected patients.

Presents, June 16, 1881.

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"On Gravimeters; with special reference to a Torsion Gravimeter, designed by the late J. Allan Broun, F.R.S." By Major J. HERSCHEL, R.E., F.R.S., Deputy Superintendent of the Survey of India. Received October 31, 1880. Read January 20, 1881.

[PLATES 8, 9.]

The present paper consists mainly of two parts, of which the second is the earlier, the first having been afterwards prepared as a historical introduction.

In August last a letter was addressed by Major-General J. T. Walker, R.E., C.B., F.R.S., Surveyor-General of India, to the Director-General of Stores for the Indian Government, enclosing copy of a letter from the late Mr. Broun on the subject of his gravimeter. The object of General Walker's letter was to trace, if possible, the instrument in question, and to secure it for the Indian Government. It was also suggested that I might be able to judge of its efficiency. I also received a communication to the same effect, and in consequence offered my services.

On enquiry it appeared that the gravimeter had become the property of the Royal Society. The loan of it was obtained and I was requested to call and see it. It was then arranged that I should take the instrument and report upon its capabilities at greater leisure than was possible at the India Office.

The gravimeter is an instrument obviously requiring very careful manipulation, and as I was not for some time after it came into my hands in possession of any description of its intended use, the examination was attended with anxiety as well as difficulty. It occupied me for three weeks, during which time I was in constant fear lest some awkwardness should prevent my returning it uninjured. I purposely, therefore, wrote what I learnt about it from day to day; and this will perhaps excuse the length of my report, which was eventually submitted to the authority from whom I received the instrument.

The course of the examination necessarily led to my becoming acquainted with the subject in several ways, but the narrative form and the immediate object of the report precluded any discussion. There appeared nevertheless to be sufficient in the study of the instrument itself, and especially of its dimensions and the weights of its parts, to justify a desire for publication. I accordingly solicited permission to communicate the report to the Royal Society, the owner of the instrument. This having been granted, it then seemed permissible to preface the report by a sketch of some of the circumstances bearing on the question of priority of invention, and other matters in connexion with the theory of the torsion gravimeter, together with remarks upon other forms of gravimeter. This now appears as Part I of the present paper.

So far as I was aware, the theory of the torsion gravimeter had not been published. In considering it, I was led to think that in some respects it involved peculiarities which might be taken advantage of to simplify the design. In anticipation of the present publication, therefore, I communicated to the Society a notice of a design to that effect, which was read on the 9th December, 1880, and appears in the "Proceedings" (vol. 31, pp. 141–146).

Although, therefore, it precedes the present communication in that respect, it will be understood to have grown out of the examination of Broun's instrument, to which I am indebted for the means of judging of its feasibility.

I. *Historical Review of Proposals or Designs for a Gravimeter.*

It is hardly necessary to describe at much length the object of instruments of this class. They aim at determining *statically* that which the differential pendulum has been had recourse to, ever since the days of Graham and of Bouguer, to determine *dynamically*, the variation of gravity at different times or places. The principle indeed is so obvious that when we recognise the fact that hitherto nothing has been effected which can be regarded as establishing a rivalry in practice, we also perceive that the interest of any historical review must centre in what it can teach as to the causes of the absence of any practical competition. For this we must first learn what forms have been proposed.

It would be hard to say to whom should be ascribed the credit of first suggesting the statical method. It would seem as if its theoretical possibility must have long been perceived. But probably no clearer enunciation of the principle is to be found than that which appeared in the first, as in all later, editions of the "Outlines of Astronomy" (Art. 234), first published in 1833.* So far as I know, what was there proposed, divested as it purposely was of everything which could confuse the principle to be illustrated, had never been experimentally tried. To understand it, a single glance at the drawing, representing a weight suspended by a coiled spring, its lower surface grazing the base of the supporting frame, should suffice. The supposed variation of gravity is intended to be balanced by the addition or removal of small subsidiary weights, the proportion of which to the principal weight will measure the relative variation. The only doubts which arise at once in considering such an instrument have reference to the means of exactly determining the contact and to the constancy of the spring. These evidently are the points towards which later improvements may be expected to be directed.

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* Art. 189 in the first four editions.

The elasticity of such a coiled spring partakes, I believe, of two characters. It would depend very much upon the *pitch* of the coil whether the elasticity would be chiefly that of bending or of torsion. If, as I suppose, these are not the same, there is to some extent a change of principle in leaving the coil, and adopting the straight suspending wire, the resistance to torsion of which is substituted (in the next form) for the resistance to extension of the coil as above. With a view to meeting one of the two difficulties mentioned, it might become a question whether the required constancy would be more attainable in the one direction or in the other. The modifications of design which actually occurred, however, do not appear to have arisen from considerations of this nature; but were aimed at securing greater facility, constancy of resistance being assumed.

About the year 1860 two inventors devoted their attention independently to methods of statical gravimetry. In each case we have means of perceiving the growth and modification of their ideas, and it must be regarded as a curious coincidence that they arrived at a nearly identical result. One of them eventually designed, and caused to be constructed, an instrument which is the occasion of this paper. The other, so far as I have been able to learn, did not give practical effect to his conceptions in the shape of a finished instrument; at least he has not published any account of observations.

The remarkable similarity which undoubtedly exists between the gravimeter as made under Mr. Broun's instructions and that which was proposed by M. Babinet, has naturally raised the question of priority. In a letter written by the former to General Walker, Mr. Broun expresses himself in a way to show that he can with difficulty divest himself of the idea that M. Babinet had been led to his design by more or less indirect cognizance of what Mr. Broun had published or communicated orally to mutual acquaintances. As it happens that I have access to correspondence bearing on the subject, I have looked into it somewhat closely, and think it not undesirable to publish some of the letters written by M. Babinet to my father at the time when he was busied about it.

The earliest publication on the subject occurs in the "Proceedings of the Royal Society of Edinburgh" for 1861, in which (pp. 411–412, February 4) Broun describes a machine, in course of construction, for the purpose of measuring small changes of gravity by the balancing of a bifilar disturbance by a *hair spring* placed below the suspended weight.

In February, 1863, appeared Babinet's design,* in which a similar disturbance is balanced by the *torsion of a single wire.* This was followed immediately by a reclamation by Principal Forbes, on behalf of Broun (absent in India), drawing attention to the prior publication, and saying that the new design is practically identical with that already published. In May of the same year appeared a letter from Broun to the Editors of the "Philosophical Magazine," enclosing translation of a letter from himself to the Perpetual Secretary of the Paris Academy of Sciènces, the object of which is likewise to claim priority of invention. He refers to his paper in the Edinburgh "Proceedings" in proof. The whole question turns, I think, on this sentence:—"We found, however, that the spring, like the balance-spring of a watch, employed under the weight, acted badly, and I substituted a simple gold wire in the same year" [1861]. Compare this with the statement in the letter of 1879, where he says "Babinet's proposal was for the same instrument exactly, and it was published more than two years after mine, and after, indeed, I had described the instrument to different persons in Paris." It is clear that Broun did not perceive that there was a real difference between the two forms in which the elasticity of metal was opposed to the force of gravity, and that he had *published* nothing regarding his use of *the torsion wire* at the time of Babinet's design. It was only towards the close of 1861, at the earliest, that Broun introduced the gold wire in place of the spring, and as the letters which I shall give prove Babinet to have been working out the same idea early in 1862, there is no doubt some show of reason in Broun's complaint, when he says (as he does in his letter of 1863) that not only was his instrument seen by many persons in Adie's shop in 1861, but that the gold wire which he substituted was actually recommended and supplied by a Parisian artist. Still I think it impossible to read the following letter, and retain any suspicion that Babinet was conscious of having borrowed the principal part of his design from another. The letter is interesting enough in itself, apart from this question.

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* "Comptes Rendus," lvi, 1863, pp. 244–248; and "Cosmos," 1863, pp. 177–181.

"Paris, ce 3 Février, 1862.

"*Sur la Mesure statique de la Pesanteur.*

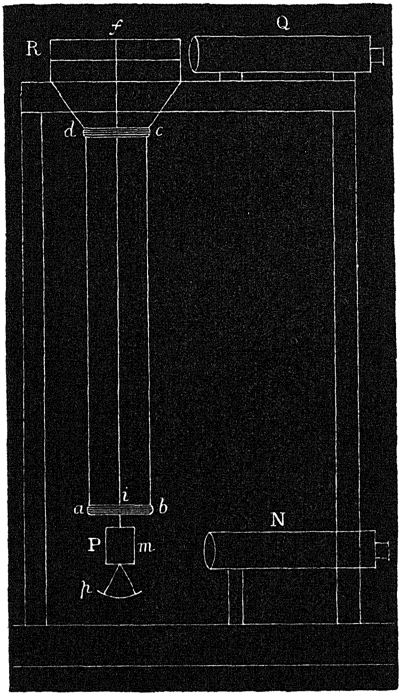
"MONSIEUR ET ILLUSTRE CONFRÈRE,

"C'est une des plus importantes idées qui aient été émises pour la connaissance de notre globe que celle de la mesure statique de la pesanteur. Je crois avoir résolu le problême pratique après d'innombrables essais et bien des années de réflexion. Ce qui distingue surtout ma méthode, c'est le *fractionnement* de la pesanteur, de manière à équilibrer l'effet d'un poids considérable par la torsion d'un fil métallique de force moyenne et restant dans les limites de l'élasticité parfaite. En effet je réduis le poids à $\frac{1}{200}$ de sa valeur au moyen du pendule bifilaire.

"Ayez la bonté d'examiner l'instrument que je propose, de m'indiquer des corrections, et je le ferai construire de suite. Dès que j'aurai votre réponse, je communiquerai ce travail à l'Institut en y mettant pour introduction un précis de ce qui est au No. 23 des 'Outlines of Astronomy.'

"Il n'y a dans mon appareil, ni point de départ obligé, ni mesure d'angles, ni loi supposée pour l'élasticité, ni permanence exigée, ni mesures de longueur, ni inertie de départ à vaincre, ni difficulté d'établissement ou de transport, ni mesures de temps—'it is not laborious, tedious, and expensive;' la température n'a pas le temps de changer pendant la courte durée de l'expérience, ce qui répond à votre condition d'*expédition*.

"L'effet de la température (if any) peut être facilement étudié au préalable et pris en compte.



"*bc* et *ad* sont deux fils métalliques très fins et sensiblement sans torsion (d'ailleurs leur torsion pour un angle constant s'ajouterait sans inconvénient à la force directrice du bifilaire). *bc* et *ad* ont un mètre de longueur ou 1,000 millims., *ab* et *dc* ont 10 millims.: en sorte que la distance de *a* et de *b* au point milieu *i* où s'attache le fil de torsion *fi* est de 5 millims. Soit $ia=ib=a=5$ millims.

"La tige horizontale inférieure *ab* du bifilaire porte au poids d'environ 1 kilo.=1,000 grms., en y comprenant le poid additionel *p* qu'on doit ajouter au poids P dans un petit plateau attaché au dessous de P.

"Si P est le poids que porte un pendule bifilaire, on sait qu'étant écarté de sa position de repos d'un angle α, il tend à y revenir avec une force—

$$P\frac{a}{l}\sin\alpha\sqrt{1-4\frac{a^2}{l^2}\sin^2\tfrac{1}{2}\alpha}$$

(Si a et l sont du même métal, la chaleur ne trouble point la force directrice du bifilaire, puisqu'elle dépend unique de $\frac{a}{l}$). a étant égal à 5 millims., et l à 1,000 millims., la force directrice du bifilaire n'est qu'une fraction, savoir $\frac{5}{1000}$ ou $\frac{1}{200}$ de sa grandeur naturelle. Du reste on verra tout-à-l'heure qu'il n'est besoin de mesurer ni a ni l.

"Supposons la tige supérieure *dc* du bifilaire déviée de 90°. Alors, le poids P étant de 1,000 grms., la force directrice du bifilaire pour $\alpha=90°$, sera à peu près $\frac{1}{200}\cdot1{,}000$ grs.=5 grms. On équilibre cette force directrice horizontale par une torsion de 180° que l'on donne au fil métallique *fi* attaché en haut à la pièce tournante R et en bas au point *i* milieu de *ab*. La tringle supérieure *dc* du bifilaire tient aussi à une autre pièce rotative faisant partie de R et qui permet de tourner *cd* de 90°. Ces deux pièces de R tournent l'une dans l'autre, à l'ordinaire.

"La lunette inférieure, N, a des fils micrométriques qui vont se mirer en *m* sur un petit miroir vertical fixé au poids P. En ramenant l'image de ces fils à la coïncidence avec les fils eux-mêmes, comme dans le pointé nadiral, on ramène la direction *ab* de la partie inférieure du bifilaire à sa position primitive. De même, des miroirs attachés aux parties tournantes en R, et réglés sur la lunette Q, assurent des rotations connues pour le haut du bifilaire *dc* et pour le fil de torsion *fi*. Voici maintenant l'opération complète :—

"L'appareil étant calé par ses niveaux, et bien stable, j'amène le bifilaire dans la position où l'image des fils de la lunette N coïncide exactement avec ses fils. À ce moment un petit miroir vertical fixé latéralement à la pièce qui en R donne le mouvement de rotation au bifilaire, ce petit miroir, dis-je, renvoie la partie inférieure des fils de la lunette Q sur eux-mêmes ; et de plus un autre petit miroir pareil, fixé au-dessus du précédent à la pièce qui donne la rotation au fil métallique *fi*, renvoie dans la partie supérieure du champ de la lunette Q l'image de la moitié supérieure des fils de cette lunette sur ses fils eux-mêmes.

"Tout étant ainsi réglé :—

"Je fais tourner de 90° la pièce qui guide la tige supérieure *dc* du bifilaire, ce que l'on obtient au moyen d'un second petit miroir vertical fixé latéralement sur cette pièce et à 90° azimuthalement du premier. Ce miroir est amené à remplacer le premier miroir de la même pièce pour le renvoi des rayons dans la lunette Q, et la rotation de *dc* est alors de 90°.

" La pièce tournante à laquelle est fixée l'extrémité supérieure *f* du fil de torsion a aussi un second petit miroir vertical qui fait azimuthalement avec le premier miroir de cette pièce un angle égal à trois fois 90°, et en tordant par son moyen le fil *fi* de trois fois 90° en sens contraire du mouvement donné à *dc* du bifilaire, la torsion de *fi* restera en définitive une torsion de 180° quand sa direction *ab* du bifilaire aura repris sa position primitive.

" Soit P le poids fixe du bifilaire (y compris *a*, *b*, &c.). On ramenera *ab* à sa position primitive au moyen d'un petit poids additionnel *p*. Alors la pesanteur *locale*, agissant sur une masse $P+p$, par l'intermédiaire du bifilaire équilibre la torsion de 180° du fil métallique *fi*. Si dans une autre latitude il faut $P+p'$ pour équilibrer la même torsion, les intensités de la pesanteur dans les deux localités seront entre elles comme $P+p$ est à $P+p'$.

* * * * * * * *

"Pour $\frac{1}{10000}$ (qui est réclamé dans l'article des 'Outlines') voyons l'effet produit par un poids de 0·1 gr., ajouté à un poids de 1,000 grs. Ce poids de 1,000 grs. équivaut dans le bifilaire à 5 grs., et un poids de 0·1 gr. équivaudra à $\frac{0\cdot1}{200}=\frac{1}{2000}=0\cdot0005$ gr.

"Mais 5 grms. sont équilibrés par une torsion de 90° [180°] du fil *fi*. Donc 1 gr. correspondrait à une torsion de 36°, ou bien de 129,600″. Alors 0·001 répond à une torsion de $129''\cdot6$, et $\frac{1}{2000}$ gr. correspond à la moitié de ce nombre; mais comme le mouvement du miroir double le déplacement de l'image, il restera un déplacement de $129''\cdot6$ (un peu plus de 2′) pour $\frac{1}{10000}$ de variation dans la pesanteur. Si la lunette N a un peu plus de 30 centims., le déplacement de l'image des fils sera de $\frac{1}{5}$ millim.

" *Nota.*—Il faudra prendre la position de *ab* ramené à sa position primitive en faisant osciller *ab* pour éviter les adhérences de départ.

" Voyez, jugez, et surtout conseillez ; votre avis sera suivi. Je finis par ces mots des 'Outlines': 'The great advantages . . . render the attempt well worth making.'

" J'ai l'honneur, &c.,

" BABINET.

"À SIR JOHN HERSCHEL.

" P.S.—Il faut que je me hâte de publier ce que j'ai trouvé, car à 69 ans ce serait vraiment être trop exigeant envers la bonne providence que de lui demander encore indéfiniment du temps pour remplir la

tâche qu'il lui a plu de m'assigner. Vous avez été plus sage, pour le grand bien de la science. Moi, je puis malheureusement me dire,

> "'Cras vives! hodiè jam vivere, Posthume, serum est.
> Ille sapit quisquis, Posthume, vixit heri.'—MARTIAL."

I have also seen a copy of the reply to this letter. It is needless to say that it was full of approval, and only made some suggestions, which were at once adopted or accepted. There was no mention of any other form of gravimeter having been proposed in England. I need not transcribe from the subsequent letters; until, on the 15th February, a year later, Babinet again writes :—

"M. Le Verrier me presse de terminer notre balance gravimétrique. Je ne veux pas commencer imprudemment avant d'avoïr votre ultimatum d'approbation."

Here follows a *résumé* of the principles involved in the design, as to which I need only remark that the elasticity of the suspending wires is not taken into account, nor is the necessary effect of this elasticity upon the balance in any way referred to.

"L'appareil sera établi et essayé à l'Observatoire Impérial de Paris, puis essayé dans les latitudes voisines. Je serai heureux si cette mesure de la gravité et les déplacements nécessaires qu'elles entrainera me fournissent l'occasion de vous voir en Angleterre."

The letter then passes on to other matters—a new plan for measuring aberration by means of gratings—and only returns to the gravimeter at the close.

"J'aurais encore attendu à vous parler de ceci sans la circonstance de notre balance gravimétrique. En cas de succès de cette balance pourriez-vous, vous ou quelqu'un des vôtres, vous charger des stations du Royaume Uni? Il faudrait au moins une mesure de gravité pour chaque degré carré de surface.

"*Nota.*—Sitôt après votre réponse on se mettra à l'œuvre pour l'appareil gravimétrique.

"Recevez, &c.,
"BABINET."

The paper in the "Comptes Rendus" (lvi, pp. 244—248) was read on February 9, 1863, or a week earlier than the date of this last letter, which therefore appears to have resulted from the approval with which the paper was received at the reading. The published account adds little to what the letters tell—on the contrary—which is partly my reason for transcribing them. It is noticeable that there is from first to last no mention of any prior attempt in the same field.

I have endeavoured to ascertain, by enquiry from a gentleman who was associated with Babinet in physical researches of this nature, whether the instrument was ever constructed; but I do not gather from the reply that such was the case. It would seem as if there was too much uncertainty as to the constancy of the force relied upon in torsion.

The original paper communicated by Broun to the Royal Society of Edinburgh follows immediately upon two others by the same author, in one of which the theory of the bifilar magnetometer is considered, and *mention is there made of the elasticity of the suspending wires.* Their effect is considered to be of little importance. The effect of temperature in modifying the elasticity is also alluded to. We cannot therefore suppose Mr. Broun to have disregarded these considerations altogether. It is perhaps the more remarkable that he has left no indication of being alive to the really interesting balance which occurs in his instrument, and which must equally have occurred, and can hardly have escaped his notice, in the former one.

In his letter, above referred to, he speaks of his gravimeter as if the instrument ultimately perfected was substantially the same as the one described in 1861. But, as I have had occasion to point out, it differs in one respect which I cannot but consider important—the difference between a spiral balance spring and a twisted wire. It is to be regretted that we are not in possession of any information as to the reasons which induced him to discard the one in favour of the other. In any case, the change is distinctly a part of the history of the invention. The next step was the construction of the instrument as mentioned in his letter; and then its exhibition and description by himself in the Catalogue of the Loan Collection of Scientific Instruments at the South Kensington Museum, where, he tells us suggestively, it was shown alongside of Mr. Siemens' bathometer. We may now turn our attention to this last.

My knowledge of the bathometer designed by Dr. C. W. Siemens is confined to the account given of it in the "Philosophical Transactions" for 1876, and I should not presume to examine it were it not necessary to do so for the purpose of this review. Dr. Siemens, it is true, regards it, in the introductory passages of that paper, as having a different sphere of action; but, on a careful consideration of the principles—so far as I understand them—of its construction, it appears to me to belong decidedly to the class of instrument, whatever we may call them, typified by the one already mentioned as described in the "Outlines of Astronomy." The special purposes for which it has been designated, and the actual uses to which it has been put, do not of themselves preclude it from being set to the more general purposes and uses of a gravimeter.

Reference is made in the cited paper to another form of instrument constructed by the author in (or about) the year 1859, which, however, was abandoned, although it enabled him "to predict approximately the depth that would be found on the use of the sounding line." In this instrument the specific gravity of mercury would seem to have been measured by determining the column necessary to balance the pressure of a fixed quantity of confined air. The rise of the surface under a change of gravity was magnified by a lighter supernatant liquid being forced up a narrow tube. The principle, in this case, is analogous to the former typical one only if we regard the compressed air as acting the part of a spring. It is obviously dependent on the possibility of guarding against, or exactly allowing for, the change of elasticity with temperature—in this, too, being analogous to the typical form. It was sought to meet this very serious difficulty by keeping the air at the temperature of melting ice. There is another method—perhaps not less difficult to practise successfully; that, namely, of selecting three (or preferably four) more accessible temperatures, and so experimenting at these that in the long run the actual observations shall supply a constantly accumulating body of evidence as to the requisite factor, as well as two or more actual data in each case from which to infer what is required. But it might turn out after all that the uncertainty must remain greater than the variation to be measured. The possibility of such a result militates strongly against methods involving large corrections. But, perhaps, least of all in cases such as this where the whole instrument may be immersed without injury. It then becomes chiefly a thermometric question.

In the bathometer, on the contrary, which superseded the one just mentioned, "changes of temperature are entirely eliminated from the result," at least in theory. In practice such a thing would perhaps be hardly possible; but in any case residual effects may be regarded as subjects for ultimate consideration. I will now endeavour to condense a description of this instrument, so as to assign it a place in the present category.

There are two spiral steel springs which, in the drawing of the smaller of two instruments, are about 16 inches long and rather more than one in diameter. The lower ends are attached to a cross-bar which is to bear the intended weight. This weight is that of a column of mercury resting upon a certain surface to be presently described. There is an arrangement for maintaining the height of the column constant. I am not sure that I understand how this works; but supposing it effective, the weight upon the cross-bar must depend upon the area pressed by the mercury; and if this also is constant we have in effect the same principle as in the gravimeter depicted in the "Outlines."

The mercury is contained in a vertical steel tube having cup-shaped enlargements above and below. The lower end is closed by a flexible corrugated steel plate, which has a solid central disk, by which the pressure on the plate is transferred to the cross-bar. When this plate is horizontal, the mercury presses equally on every part; but it is clear that the whole of the pressure is not borne by the cross-bar. In fact, it would only by trial be possible to ascertain what weight is borne by the springs. Any change in the position of the disk must alter the curvature of the plate, and with it the proportion of the weight borne by the springs and by the rim of the cup—irrespective of any change in the height of the column. The diameter of the steel tube would seem to be adjusted so as to cause a proper change in the level of the upper surface, when the bulk of the mercury is affected by change of temperature. I am not quite sure that, in presence of these several causes of variation, the principle of constancy of weight on the springs can be said to be retained.

Alterations of position of the disk are read by means of a micrometer below the cross-bar. It seems clear that the desired indications must be obtained from the readings of this contact micrometer, and some curiosity may be experienced as to the manner of interpreting its readings. On this point the author says:—"It would be difficult to determine the actual scale of the instrument *à priori;* and I therefore adopted the easier and safer method of relying for its final adjustment upon the result of actual working."

If it were possible to ascertain, with some approach to certainty, the weight borne by the springs in some particular position of the diaphragm and cross-bar (to be called the zero position) the instrument might be used, on the principle of the simple gravimeter, by ascertaining *what weight* upon the cross-bar would be necessary, under a diminished gravity, to bring it again to this zero position—temperature being the same. This, however, is not what is intended in this form of instrument.

The subject of compensation for temperature receives considerable attention. This, indeed, is very necessary, when we remember the sensitiveness of springs to heat. The author gives, as the result of experiment upon these springs, a factor of variation which appears to be $\frac{1}{4000}$ for each degree Centigrade. The importance of temperature in such case will be better understood if we consider that at this rate it would need but 21° C. to effect as great a change in the elastic force, as transference from equator to pole would effect in gravity. Change of temperature has here, it would seem, an effect twelve times as great as it has in the case of the pendulum. Certainly there is room for compensation; and Dr. Siemens obtains it, to some extent, by apportioning the diameter of the vertical tube to the cups. It is not necessary, for the present purpose, to consider closely how this exercises a compensating effect. It is estimated, by experiment, that the factor is reduced to $\frac{1}{800000}$. It seems highly probable, however, that the residual effect would no longer be a simple function of the change of temperature, and might be quite incalculable.

It is also necessary to consider that the masses of metal concerned would probably, except under controlling conditions, cause considerable uncertainty in this effect.

Enough has been said to show that the bathometer, although a gravimeter in principle, cannot be regarded as likely to prove of service in measuring small changes of gravity under different climates. Its efficacy as an instrument for measuring sea-depths need not be considered here. It may have peculiar properties in that connexion which I have not understood; for indeed I may confess a certain hesitation in believing that any instrument can do that, by determining the change of attraction, except under conditions favouring an empirically deduced scale.

There are rumours of other designs for measuring small changes of gravity, of quite recent invention, of which I can only say at present that it is very much to be hoped that they will serve their intended purpose, and that we shall soon be in possession of experimental evidence to that effect.

II. *On the Torsion Gravimeter, constructed on the Design of the late J. Allan Broun, by Dr. C. S. Müller, of Stuttgart.*

Before entering upon any description of this instrument it may be well to explain that the title of this paper is intended to take cognizance of the fact that the designer did not experiment with it or improve upon it after it left the maker's hands, and that consequently some portion of the merit, whatever it may prove to be, is due to the appreciation of the design by the latter. Conversely, it is due to the designer to recognise that failure of any kind may to some extent be attributable to misapprehension on the part of the maker. At the same time, it must be said that the workmanship is of a high order, and betokens a more than ordinary attention as well as great skill and delicacy. Should the instrument justify its existence by its ultimate utility, it cannot be denied that it will owe its success, in a high degree, to the intelligence of the constructor as well as to the genius of the inventor.

There are two ways of describing a new form of instrument. We may either approach it from outside, and learn its functions by considering its parts, or we may study it with a prior knowledge of its intention. The latter method is, perhaps, the best for one to adopt who wishes to describe an instrument of his own designing; for he cannot fail to indicate truly what design he had. But for one whose only knowledge of the functions of an instrument are inferred from what he sees, it might be dangerous to presume a full knowledge of the design. I shall, therefore, describe the instrument as I see it, with direct reference to what I conceive to be the intention.*

The gravimeter stands, when set up for use, on a *tripod;* but it might equally be stood upon a table with a bay cut out of the side two or three inches deep. In either case it would stand on three adjusting *foot-screws* carrying a thick brass *foundation plate.* Below this plate projects what I will call the *well* and a *cathetometer.* Above it stands the *chamber*, consisting of two brass plate sides and glass plate front and back, besides various clamping and manipulating appliances. On the top of the chamber are two cross *levels*, and from the centre rises a hollow *shaft.*

Measuring from the surface of the foundation plate, the depth of the well is about 8¼ inches (26 centims.), and the height of the chamber 5½ inches (16 centims.). The shaft stands about 18½ inches (47 centims.) above the plate, or 13 inches above the roof of the chamber. The latter is about 2¼ inches wide and 2⅜ inches deep (say 6 centims. each way). This general description will suffice to give an idea of the framework. More minute descriptions will follow as they become necessary.

Within the chamber are suspended by wires, one below the other, two weights, which for distinction I will call the *major weight* and the *minor weight.* The major weight consists of a brass cubical *block*, from which rise two lateral rectangular *pillars*, crossed at top by a somewhat slighter *bar.* The block is perforated vertically by a large cylindrical hollow. This major weight is suspended by two parallel wires which traverse the shaft; being just visible at its lower end, where they enter two small screws on the cross-bar. There are means of adjusting both their length and distance apart, above and below.

The principle here is that of the bifilar torsion balance: if the block be turned through any angle less than 180°, its tendency to return is (mainly) due to the weight of the mass suspended. This, however, is not that of the major weight only; for from the under side of the cross-bar hangs a single wire, which, descending through the hollow in the block, is attached to and supports the minor weight; hence the mass suspended by the double wire is the sum of the masses of the two weights.

The minor weight consists of a light frame, which I will call the *head*, and a long thin glass rod or *plunger*, which descends into the well.

Suppose the major weight turned through an angle θ. When the disturbance has subsided and oscillations have been quelled, it will, of course, be found that the minor weight has turned through a like angle. The double wires are in a state of torsion, but the single wire hangs freely, helping by its tension to increase the tension on the double wires caused by the major weight, but not itself in a state of torsion. Now, suppose the minor weight turned round : it will immediately begin to exert through the resistance to torsion, or elasticity, of the single wire, a force which will tend to relieve or oppose, according to the direction of the new application, the external force which keeps the major weight detorted. Suppose the new force applied in the *same* direction, so as to *relieve* the former. As the torsion of the single thread increases—with the increase of the angle through which the lower weight is turned—a point is at length reached when it exactly relieves the whole of the external force applied to the upper. Suppose this to occur when it has turned through an angle ϕ, *i.e.*, through an angle $\theta+\phi$, from the initial position. Then it is clear that the force which turned the upper end of the single wire through an angle θ, has been found equal to that which turns its lower end through an angle ϕ, relatively, and $\theta+\phi$, absolutely. We have now to consider what these forces are.

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* Since this report was written, two plates have been drawn to illustrate the description which follows. Necessarily there is no direct reference to them in the text.

Before doing so, it would be advisable to recognise the means provided for observing these angles.

In some way, which there are no means of exactly discovering, the designer or constructor has ascertained that by a certain apportionment of lengths, weights, and thicknesses, the proportion of θ to ϕ (which of course is a variable one) can be made 1 : 3 when $\theta=90°$. The result of this is, that when $\theta+\phi$ amounts to one complete revolution, θ alone is one quarter of a revolution. [In this position the resistance of a bifilar suspension is a maximum. I do not know that it has any strong advantage except what may turn on that. It is only necessary to allude to the fact, to take occasion to add that it is certainly of no importance, either practically or theoretically, whether θ is exactly or only approximately, equal to 90°. We shall see eventually that another consideration (perhaps not considered by the inventor) entirely overrides the one mentioned. This by way of parenthesis.]

The head of the minor weight carries a small flat *mirror*, which faces in the position of rest, as also after one revolution, a horizontal *collimator*. The block of the major weight carries *three similar mirrors*, of which one is parallel to the former (or may be made so) when in a state of ease, and the others become so (under proper conditions) when the major weight has been turned to the right or left through one quarter of a revolution. These mirrors face the upper half of the object-glass of the collimator. A *fiducial mark* in the focus of the latter is seen by reflection from the mirrors when they are perpendicular, or nearly so, to the line of sight. It will be necessary to return to this in describing the intended observation.

To understand thoroughly the conditions of equilibrium we must now study in detail the counteracting forces. We have seen that the weight supported by the double wire is the sum of the two weights described as the major and the minor weights. One-half of this sum is, of course, borne by each of the wires, which are further twisted individually through a quarter of a circle. Let R be the length of these wires, and $2r$ their distance apart. Consider one only: the upper end being fixed, the lower will describe a curve which will be the intersection of a sphere by a vertical cylinder, the ordinate from which to a plane drawn horizontally through the lowest point may be shown to be equal to

$$\frac{r}{\mathrm{R}} \cdot r \cdot \operatorname{versin}\theta \left\{ 1 + \tfrac{1}{2}\frac{r^2}{\mathrm{R}^2} \cdot \operatorname{versin}\theta - \&\mathrm{c.} \right\}.$$

As $r : \mathrm{R}$ is a small fraction, less than $\frac{1}{100}$, it will be a question whether the second and further terms may not be neglected; for the present we may be content with the first, as an approximation only is wanted here. This ordinate is the height through which the weights rise as they are turned round. Let this be called h. A little consideration of the meaning of $\frac{h}{r \operatorname{versin}\theta} = \frac{r}{\mathrm{R}}$, *a constant*, shows that in this case the restriction to the first term makes the path of the lower end of the wire a circle lying in an inclined plane,* and the force tending to cause rotation (irrespective of the torsion of the parallel wires) is that of a body having the joint weight of the two, whose path of descent is to be along this circle. The gradient along this path (which is the tangent of the inclination) is $\frac{dh}{r \cdot d\theta} = \frac{r}{\mathrm{R}} \sin\theta$, which is zero at 0° and 180°, and a maximum at 90°. Let $\mathrm{A}+\mathrm{B}=\mathrm{P}$ be the weights, which are augmented for adjustment by a small weight, p. Then the force, depending on gravity, which tends to turn the system, resolved horizontally, is $(\mathrm{P}+p) \tan \angle$ inclination $=(\mathrm{P}+p)\frac{r}{\mathrm{R}}\sin\theta$.

Neglecting for the present the torsion of the parallel wires, we see that this force is resisted by the torsion of the single wire, as to which we must have regard to two propositions:—

(1.) Torsion is independent of tension;

(2.) Torsion varies directly as the angle of twist.

We may, therefore, express the torsion by $\rho_1\phi$ (at the distance unity), remarking that ϕ is mechanically increased until it balances the force tending to turn the system as above described.

Now, since $(\mathrm{P}+p)\frac{r}{\mathrm{R}}\sin\theta$ attains a maximum when $\theta=90°$, if at this point it were balanced by $\rho_1\phi$, any increase of torsion caused by increase of ϕ, however small, would be answered by no corresponding augmentation of the opposing force. The upper system would obey, θ would increase, but the resistance would diminish instead of increasing. In short, 90° would represent a position of *unstable* equilibrium.

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* This demands an elliptical cylinder.

This state of things is prevented by the torsion of the parallel wires, which act in aid of the weight and *defer* the condition of unstable equilibrium to a point some degrees beyond 90°. The perception of this may or may not have been present to the designer; but it may obviously be made use of advantageously as follows:—Let $2\rho_2\theta$ denote the torsion of the two parallel wires when turned through an angle θ. Then the forces which balance are $(\mathrm{P}+p)\frac{r}{\mathrm{R}}.\sin\theta+2\rho_2\theta$, and $\rho_1\phi$; and the utility of the instrument, depending in the first place on the absolute constancy of $\rho_1\phi$, depends also on the possibility of exactly equalising to this the other force by varying either θ or $\mathrm{P}+p$. It is in this respect exactly analogous to a balance, the desired relation of θ to ϕ being equivalent to the demand for horizontality in the latter. The sensibility of such an equipoise will be measured by the smallness of the change in $\mathrm{P}+p$ requisite to produce, or to correspond to, a given small change of θ.

Let $$(\mathrm{P}+p)\,\frac{r}{\mathrm{R}}.\sin\theta+2\rho_2\theta=\mathrm{F},$$

then $$\frac{d\mathrm{F}}{dp}=\frac{r}{\mathrm{R}}.\sin\theta,\qquad \frac{d\mathrm{F}}{d\theta}=(\mathrm{P}+p)\,\frac{r}{\mathrm{R}}.\cos\theta+2\rho_2,$$

and $$\delta p=\frac{\mathrm{R}}{r}\ \mathrm{cosec}\,.\,\theta\left((\mathrm{P}+p)\frac{r}{\mathrm{R}}.\cos\theta+2\rho_2\right)\delta\theta.$$

When $\theta=0°$, δp is infinite; *i.e.*, no change of weight will affect θ. Similarly when $\theta=180°$. But when $\theta=90°$, $\delta p=2\rho_2\frac{\mathrm{R}}{r}\delta\theta$. At this point no relation would subsist between δp and $\delta\theta$ but for ρ_2; *i.e.*, if the equilibrium were independent of the torsion of the parallel wires. As it is, the equilibrium is a compound one, and is disturbed by a change of weight, *the effect of which is inversely proportional to the torsion of the parallel wires*, in apparent contradiction to the first of the above propositions regarding torsion. If the torsion of these wires be doubled, the addition of the same small weight δp will cause a change of θ to one-half the amount. The sensibility of the instrument is therefore proportional (within limits) to the fineness of these wires; but this must not be carried so far as to bring the position of unstable equilibrium too near to 90°, or else the stability of the balance will become too delicate and observation too difficult.

To determine the position of unstable equilibrium, consider the general statical equation

$$(\mathrm{P}+p)\frac{r}{\mathrm{R}}\cdot\sin\theta+2\rho_2\theta=\rho_1\phi.$$

When the left-hand side is a maximum its differential with respect to θ equals zero, therefore

$$(\mathrm{P}+p)\frac{r}{\mathrm{R}}\cos\theta_0+2\rho_2=0,$$

whence

$$\cos\theta_0=-\frac{\mathrm{R}}{r}\cdot\frac{2\rho_2}{\mathrm{P}+p}.$$

Now, as in the intended position of rest, $\theta=\frac{\pi}{2}$, $\phi=\frac{3\pi}{2}$, we have

$$(\mathrm{P}+p)\frac{r}{\mathrm{R}}=(3\rho_1-2\rho_2)\frac{\pi}{2}=2\rho_2\left(\frac{3}{2}\cdot\frac{\rho_1}{\rho_2}-1\right)\frac{\pi}{2};$$

Combining this with the former, it appears that

$$\sec\theta_0=-\left(\frac{3}{2}\cdot\frac{\rho_1}{\rho_2}-1\right)\frac{\pi}{2}.$$

The double wires and the single wire are no doubt of the same material, but the former are thinner. Their lengths are as 4 : 1. Suppose their thicknesses as 2 : 3. Then $\rho_1:\rho_2::4\times3^2:2^2::9:1$, which gives

$$\sec\theta_0=-12{\cdot}5\frac{\pi}{2}=-19{\cdot}64,$$

and ∴

$$\theta_0=92^\circ\ 55'.$$

That is to say, the position of unstable equilibrium lies less than 3° beyond the intended position of rest. [This is closer than I should have thought, but it is borne out by the fact that I have not been able (with the existing adjustments) to reach the intended position of rest *both* ways, *i.e.*, turning both to the right hand and to the left hand, before reaching the point of unstable equilibrium. This is easily accounted for by supposing the primary position to be a balanced one, a slight deviation from parallelism of the mirrors being induced by a slight initial torsion of the parallel wires.] To return :—

The angle through which the *lower* end of the single torsion wire is turned, by the stop acting on the minor weight, is $\theta+\phi$. As this angle approaches 360°, θ, which is the angle through which the *upper* end and the major weight are turned, approaches, or should approach, 90°. As this stage is approached a very small increase of $\theta+\phi$ induces a larger and larger increase of θ, according to the proximity to the value θ_0. And the above expression for sec θ_0 shows that this depends on the ratio of $\rho_1:\rho_2$. By varying the strength of the double wires the position of unstable equilibrium may be placed anywhere from $\theta_0=90°$ to $\theta_0=180°$; and the nearer it is to 90°, *i.e.*, the weaker they are, the more sensitive will the instrument be at the position $\theta=90°$. This suggests, I think, that the double wires should be coarse and inelastic rather than the reverse.

The ratio $r:\mathrm{R}$ is as nearly as I can judge by measurement 1 : 138, and $(\mathrm{P}+p)$ would be, at the equator, 3,725 grs.; so that $(\mathrm{P}+p)\frac{r}{\mathrm{R}}=$ 27 grs. very nearly. Now,

$$\rho_1\,.\,\pi=\frac{2}{3}(\mathrm{P}+p)\,\frac{r}{\mathrm{R}}\,.\,\frac{1}{1-\frac{2}{3}\,.\,\frac{\rho_2}{\rho_1}}$$

$$=18\left(1+\frac{2}{3}\,.\,\frac{\rho_2}{\rho_1}+\ldots\right)\text{ grs.}$$

This is the force, exerted at a distance $r=\cdot09$ inch, which is requisite to turn the single wire, of length 3 inches, through the angle π. If $\frac{\rho_2}{\rho_1}$ be taken as above, equal to $\frac{1}{9}$, $\rho_1\pi=19\frac{1}{3}$ grs., at ·09 inch; and $\rho_2\pi=2\frac{1}{7}$ grs. nearly.

We can now form some estimate of the effect of a small change of weight. We have seen that $\delta p=2\rho_2\,.\,\frac{\mathrm{R}}{r}\,.\,\delta\theta$. Substituting numerical values, as above, we find $\delta p=138\times4\frac{2}{7}\times\frac{\delta\theta}{\pi}=591\frac{\delta\theta}{\pi}$ nearly.

The collimator contains a scale, exactly $\frac{1}{4}$th of an inch long, divided into 30 parts. The focal distance being 8·5 inches, the angular value of the scale is $\tan^{-1}\frac{1}{34}$ or 1° 41′; and of each part, 3′ 22″, or $\frac{1}{3200}\pi$ approximately. Putting one of these parts equal to $2\delta\theta$ (doubling on account of the reflection) we find $\delta p=\frac{591}{6400}=\cdot092$ gr., the change of weight which will deflect the mirrors *relatively* one division. It follows that the 30 divisions of the scale correspond to a change of weight of about 2·76 grs.

But we must remember that this relation depends on an assumption, viz., that $\rho_1:\rho_2::9:1$. To provide for a more accurate estimate being hereafter obtainable, let $\frac{2\rho_2}{3\rho_1}=k$. Then $\rho_1\pi=\frac{18}{1-k}$, $\rho_2\pi=\frac{27k}{1-k}$, $\delta p=$ $7452\,\frac{k}{1-k}\,\frac{\delta\theta}{\pi}$; and in the case of the scale-division $\delta p=1\cdot16\frac{k}{1-k}$ grs., or very nearly $35\frac{k}{1-k}$ for the whole scale.

Let us now consider what may be expected as the actual consequence of a change of gravity. The most practical evidence of this is the effect on the rate of a pendulum. A pendulum which would beat seconds at the equator would gain 225 seconds, or beats rather, at the poles. The corresponding increase of gravity is in double this proportion, *i.e.*, 450 on 86,400, or 1 on 192, nearly. Weights, as measured by a constant force such as the torsion of a wire is supposed to be, are affected in like proportion but inversely. Now the weight of the mass suspended by the double wires is 3,725 grs.; and the change of weight, if one may so express it, from pole to equator, will be $\frac{3725}{192}$, or 19·4 grs.

We have seen that the range of the scale in the collimator will measure a deflection due to a change of weight of 2·76 grs., or thereabouts. It follows that the extreme change of gravity which can be looked for would cause a deflection about seven times as great as what the scale will measure.

This is provided for by the auxiliary weights, of which there are five, weighing 4·4 grs. each, and by the contrivance of the glycerine well, which will now be described. Before quitting this part of the subject, however, I should point out that one division of the scale seems to correspond to a change of pendulum rate of $\frac{\cdot 092}{19\cdot 4} \times 225 = 1\cdot 07$ second *per diem*.

The buoyancy of the glycerine in the well depends on the volume of liquid displaced by the plunger, and therefore on the diameter of the latter, which can only be got at awkwardly, in one place. To ascertain this diameter I cut a slit in a piece of zinc plate, rather wider at the mouth than at the inner end, and used it as a gauge, marking the place where the narrowing width fitted the plunger. A piece of copper wire, ·05 in diameter, was found to fit it at the same spot; perhaps ·048 would be more correct.

The specific gravity of glycerine is 1·26; hence a cubic inch weighs 349·4 grs. With these data we find that one inch rise of glycerine will buoy 0·628 gr. The cathetometer scale being divided metrically, this corresponds to 0·0247 gr. per millimetre. The micrometer reads to hundredths of a millimetre.

The riders weigh, as nearly as I can determine, 4·40 grs. each. Hence one rider has the same effect as 178 millims. of rise; but the scale only runs up to 150 millims., so that additional riders will be requisite.

I found by trial that 100 millims. rise of the glycerine buoyed about 2 grs. It was not a careful observation, but agrees fairly with the above calculation; so that it seems pretty certain that the riders are too heavy.

The total weight being 3,725 grs., the variation of gravity from pole to equator (being as 193 : 192 nearly) will require an augmentation of 19·4 grs. This corresponds to 225 seconds, therefore 1 second corresponds to 0·0867 gr., or 3·5 millims. of glycerine, if the above is correct.

We must look for accuracy which may be represented by ±0·1 second, or the instrument is not worth testing severely. This corresponds to 0·35 millim. of glycerine, or 35 divisions of the micrometer.

The foregoing description and investigation represent what I had learnt about the gravimeter by inspection, measurement, calculation, and reflection, before meeting with any instruction from the designer. In his letter to General Walker, Mr. Broun mentions having seen and corrected the proof of the description which was to appear in a new edition of the Catalogue of Instruments exhibited in the Special Loan Collection. Not expecting that a catalogue description would be otherwise than brief, and confined to principal features such as I could not fail to perceive unaided, I did not wait until I could procure it, to study the instrument. Some delay also occurred before I received, through the courtesy of the Secretary of the Science and Art Department, the extract in question. It seems right to mention this, in explanation of the independence which will be remarked in what I have said. Whereas, in what follows, I acknowledge the said description as an additional source of information. Mr. Broun's description is accordingly inserted here; and I shall then add some comments upon points which seem to need further elucidation or notice.

> "**421d. Gravimeter.** An instrument for the measurement of the variations of the earth's attractive force, invented by J. A. Broun, F.R.S., and constructed from his drawings by Dr. C. Müller, of Stuttgart. *J. Allan Broun, F.R.S.*
>
> "The instrument consists of a weight suspended by two gold wires; a single wire fixed to the top of the weight and passing through its centre carries a cylindrical lever; when the lever is turned through 360° at the normal (say southern) station, the torsion of the single wire thus produced carries the weight round through an angle of 90°. The forces then in equilibrium are, the torsion force of the single wire and the attraction of the earth on the weight, which, as the two wires are no longer vertical, has been slightly raised and seeks to attain its lowest point.
>
> "On proceeding from a southern to a more northerly station the earth's attraction increases; the amount of this increase may be measured in two ways:—
>
> "1st. The lever will require to be turned through *more* than 360° in order to carry the weight to the height due to turning it through 90°. (Had the station been more southerly the lever would be turned through *less* than 360°.) The difference of the angle from 360° measures the increase (or diminution) of weight.

" 2nd. By removing a small portion of the weight, equal to that due to the increased attraction of the earth, the weight can be turned through exactly 90° by rotating the lever through 360°, as at the normal station. (On proceeding south weight has to be added.)

" The following are the instrumental arrangements in order to make these observations:—

" The weight has on each of three sides, at its base, a vertical mirror (silvered, not quicksilvered); the middle mirror makes an angle of exactly 90° with the other two. The lever also carries a vertical mirror, which, when there is no torsion in the suspension wire is immediately below and in the same vertical plane with the middle mirror of the weight. A telescope, having a glass scale at the focus of the eye-piece, is adjusted so that images of the scale can be seen (one higher than the other) reflected from the middle mirror of the weight and the lever mirror. When both of these mirrors are exactly in the same plane, the middle division on the scale seen directly with the eye-piece, coincides with the same division in the two reflected images.

" By a wheel and pinion (with endless screw and clamp for delicate movement) placed below the instrument, a polished agate point can be made to act on a similar agate point fixed to the lever, so as to turn the latter through any angle. When turned through 360° the middle scale division again agrees with the image from the lever mirror. If the image reflected from one of the side mirrors of the weight does not agree also, the lever is turned through a greater (or lesser) angle than 360°, till this agreement is obtained; the difference of the angle through which the lever has been turned from 360° is obtained from the scale reading, as seen on the lever mirror.

" The following apparatus is employed for very small increases or diminutions of the weight. Suspended to and vertically below the lever is a carefully *calibrated* glass wire (1 millim. diameter), which enters a glass tube fixed below the instrument. At the lower end of this tube is a cistern containing a liquid (distilled water, or as at present, chemically pure glycerine). This liquid can be forced into the glass tube by a screw and piston (as in some barometer cisterns). The liquid is then raised till such a diminution of weight is produced by the immersion of the glass wire as to bring the mirror of the weight through exactly 90°, when the lever is turned through 360°. The length of glass wire immersed is read, by a micrometer microscope and scale, to a thousandth of a millimeter.

" Though finely polished agate points have been employed for turning the lever so as to diminish the friction, there is an additional apparatus to ensure that vertical friction has no effect on the observation at last. The lever contains a magnet; and two bar magnets, with rack-work adjustments for height, are placed one on each side of the instrument, so that by a pinion and rack movement they can be approached to the lever magnet till their force is exactly equal to the torsion force of the single wire, and the agate points are no longer in contact.

"The instrument is made to serve for latitudes differing about 10° or 15°, but an auxiliary apparatus carries five platinum rings, which can be lowered upon the weight, so as to make the instrument serve from the equator to the poles, and to any height in the atmosphere.

"There are special appliances for portability, by one of which the weight is fixed; another fixes the lever; so that strain is removed from the suspension wires, and the suspended parts cannot be shaken from their places. Levels, a thermometer, and other details fit the instrument for the most accurate observations. The suspension wires are fixed at their ends in a special manner, so that the fixed points cannot vary. All the suspended apparatus is electro-gilt."

(1.) We learn from this that the suspending wires are of gold, and that suspended parts are electro-gilt. I observe that there is a tendency to spottiness, resembling mould, on some parts of the gilded surface.

(2.) The part which I have called the "minor weight" is here designated as a "cylindrical lever." The latter term is but remotely descriptive.

(3.) The torsion of the double wires *is not alluded to* in describing the forces which balance each other. This confirms my doubt whether the very important part played by this torsion was recognised. It is an essential feature, without which the position of maximum gravity-action could not be chosen; and in making the adjustments it is impossible to disregard it.

(4.) There is no necessity for the two side mirrors on the major weight to be inclined at "exactly 90°" to the middle one. Indeed, it is scarcely possible to tell exactly what their inclination is. It is *about* 90°; and that is all that can be said or desired.

(5.) Of the two terms "telescope" and "collimator," the latter describes more correctly the function of the appliance by which the angular positions of the mirrors are observed. The scale is in the focus of the *object-glass* of this collimator, rather than in that of its *eye-piece*, though, of course, the latter is also true. The divisions and value of this scale are not mentioned. If my estimate is right, which makes one division correspond to $1^s{\cdot}07$ *per diem*, it is clear that this is a weak point, especially as the collimator is optically indifferent.

(6.) This "telescope" or collimator is said to be "adjusted" so as to perform certain functions. Unfortunately it has no means of adjustment except in a vertical plane. There is no horizontal motion; nor has the object-glass a power of focal adjustment by rack and pinion. It is *very* difficult to get a good sight of the reflected scale.

(7.) The only dimension stated in the whole description is the diameter of the "plunger" (1 millim.). Neither are the weights of the suspended masses stated. A precise knowledge of the joint weight is necessary for the calculation, even of *differential* results. By precise I mean to within 4 or 5 grs.

(8.) The plunger is described as a "*calibrated* glass wire." I imagine this to mean that the glass rod was specially made, and tested at every point. I do not see any advantage in its being of glass.

(9.) "The length of glass wire immersed"—that is to say, the length of liquid displaced—"is read . . . . to a thousandth of a millimeter." The graduation of the micrometer head enables hundredths of a millimeter to be read. The thousandths are by estimation. But this probably far exceeds the power of observation of the surface of a liquid, such as glycerine, in a glass tube.

(10.) "The finely polished agate points" appear to be of ruby-coloured glass—if ready fusion be any test.

(11.) The magnetic holder. I imagine this is better removed. It would be nearly impossible to guarantee its action being *entirely horizontal*. And the presence of a magnet as part of a mass, the weight of which is under examination, is inadmissible.

Should these comments give the impression that I wish to cavil at the description, I must reply that it is necessary for the present purpose. The question before us is not whether the instrument is ingenious; but whether it can be used for the intended purpose, in preference to other existing instruments whose use and powers are well known.

It may be said that that question can best be answered *by trying it*. Unfortunately this is not the case. It will already have become apparent that it is one of a class of instruments in which the observation is nothing but the last of a series of elaborate and difficult adjustments—adjustments which require patience and skill and no little time, all of which would be thrown away if the ultimate observation should prove abortive. I will now proceed to indicate more exactly what these adjustments seem to be.

The principle on which the instrument is designed involves, as a primary consideration, the angular rotation and ultimate angular position of two bodies. The angles θ and ϕ of the theory above explained, although unimportant in absolute magnitude, are all-important *relatively*. The prime defect of the instrument is to be seen in the insufficient optical means of noting and *recording* this relation. This might be remedied without altering the instrument in any way, by changing the collimator; so I will not dwell further on that, but pass on to the means of controlling and *adjusting* this angular position. Inasmuch as *torsion of wires* is in question, it is obvious that *the way in which their ends are held and turned* is a detail of extreme importance. Mr. Broun has alluded to this. He says they "are fixed at their ends in a special manner, so that the fixed points cannot vary." It is unfortunate that one cannot learn certainly by inspection, what the attachment is. It is probably by the pinch of a split screw, for the wires appear to pass through the axes of the holding screws. Whatever the method is, it ought to be unimpeachable. But not only should the holding be secure, it should also be easily manageable. It is of little avail to attach a fine wire to a delicate screw which can only be manipulated with caution by a steady hand, for the adjustments depend mainly on these ends of wires being turned accurately through very small angles. I regard it as a capital error of construction that the grasp of the ends of these suspension wires is made as *small* instead of as *large* as possible. This is an opinion based on wearisome experience no less than on common sense, for I have spent many hours in endeavouring to obtain the adjustment in question, with no other result than this experience and the discovery of the cause of repeated failure—as I will now explain.

The *first* adjustment required would seem to be to make the minor weight hang so that its mirror shall be parallel to the middle mirror of the major weight. To secure this the holding of the single suspending wire must be turned, either above or below. It is difficult, if not impossible, to get at the lower holding. But the upper one offers no difficulty except what is to be expected from the smallness of the parts. The necessary adjustment was at length made, approximately.

The *second* adjustment consists in so managing the torsion of the double wires, that when the minor weight (with its mirror) is turned through 360° *either way*, the major weight shall present its right or its left hand mirror *equally* short of or beyond the ultimate position. The observation in that case will consist in ascertaining what alteration of weight will bring about exact conjunction, *in either case*.

The difficulty, which I have already commented on, of giving any precise amount of rotation to the holding screws, is in this matter also so great as to make exact adjustment quite fortuitous. This will explain why I made the same adjustment *several times in succession* (on each occasion with sorely tried temper and patience) before becoming aware that there was something wrong. I traced this at last distinctly to a *want of permanence* of the *first* adjustment. I had spent from first to last not less than ten hours on these adjustments alone, which may give some idea of their uncertainty.

It was now necessary to ascertain what was the source of the instability. It must be clearly understood that there is not any part of the whole design which is of greater importance than the attainment of permanence in this part. If, after disturbance, the lower weight does not return to the *same* position, relatively to the upper, *with absolute exactness*, I see no chance of obtaining anything which can be called a result. It is literally a *sine quâ non.*

The instability might be due to one of two causes—I see no third alternative. Either the holding was insecure; or the wire was strained beyond what its elasticity would bear. I tried various plans to test this. At first it seemed clear that the holding was in fault. Then I fancied that the wire was strained. I mention this vacillation purposely, because my final conclusion, which condemns the holding after all, though more hopeful, *might* be wrong, and as it challenges the "special manner" noticed in Mr. Broun's description, it will be best not to be too certain. The following test, however, seems conclusive.

I prepared two needles of deal, about 2 inches long and as thin and light as seemed necessary. These were split at one end, and thrust upon the taut wire, so as to stand out from it horizontally. One was placed close to the upper holding, but free from contact; the other about 0·3 inch lower. The upper weight was clamped, and the lower then turned through two entire revolutions. [I did not scruple to overdo it, having ascertained from collateral experiments with other wire that a wire will bear being turned twenty or thirty times to every inch, without any other ill effect than a permanent twist.] The result was that the upper index turned through a few (8 or 10) degrees, and the lower through 70° or 80°—the latter being sensibly in due proportion to its distance: as to the former, I could not say exactly where the holding point might be. Now, the test would be in the positions to which they would return. If the wire was strained, the lower index would not return to conjunction with the upper: if the holding had failed, the *two* would not return to the starting point. The event proved the latter alternative. The lower index returned to conjunction with the upper; but both failed to return to the original position. I repeated the experiment, giving the weight only a single turn. The result was the same, in less degree: the holding had again failed, *i.e.*, had allowed the wire to turn in its socket *still further.* The screws were all firm, on trial.

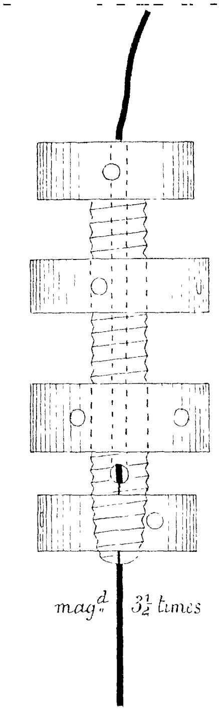
The above test is so easy, and useful, that a description of it needs no apology. Still, it is rather by way of proof that I give it; for a bare assertion of belief in the insecurity of the holding might fairly be challenged.

The above result could in no way be passed by. It was necessary to run any needful risk to ascertain further whether the evil was accidental and capable of being remedied without some decisive alteration. I therefore loosened the fastening and drew out the wire. The only result of this was to show that the wire was held *solely* by the pinch of a split screw. This is the "special manner" alluded to in the description, and I have no hesitation in saying that it is bad, because it relies on the almost microscopic accuracy of cutting of the parts. Assuming that no play is required in respect of length, there can be no objection to doubling back the wire before clamping it—and that in a large screw, split boldly for half an inch or more. No slip could possibly happen in that case, even with very slight clamping. This is the hold I have used (in the experiments alluded to), the clamp being a cleft in wood, pinched *ad libitum*.

Under the circumstances I replaced the wire, and screwed the holding nut nearly to the utmost the screw would bear; and then repeated the test—with the same result. The hold by friction (of brass upon gold) is, at any rate in this particular case, insecure. Nor do I see any means of remedy short of partial reconstruction, which should, of course, extend also to the other five holding points.

Should any doubt remain as to the slipping I add the following. Wishing to see whether it was due to the split being too coarse, I decided on extricating the holder and examining it under a microscope; as also to see if the wire showed any abrasion or destruction. To get the holder out involves a great deal of dismemberment; but the choice lay between this and leaving the question in some doubt. I succeeded in doing it—it is needless to describe how. The first result was the possibility of recognising clearly the nature of the holder, of which I give here an enlarged view. It is a screw about $\frac{3}{4}$ inch long, with capstan head, and three capstan nuts; of which two hold it on the beam, and the third pinches the split point. The split is quite flat-sided. The shank is hollow, as far as to the end of the slit, where there is a cross tunnel. By way of trial I pinched a piece of silver wire in the way the suspension wire is pinched, and twisted it. It was thinner than the latter. I think the pinch held it, for it soon broke. This was against the theory of slipping. Fortunately, I had saved a fragment of the gold wire (about $\frac{1}{2}$ inch) which had projected at the free end and had been broken off by the insertion of the pin (I suppose). This piece I inserted and pinched firmly, as firmly as I dared without spoiling the screw. The projecting end I bent into a crook, which I grasped with tweezers. I then turned the screw. Would the wire strain, or break, or slip? That was the question. The event justified previous experience. It slipped freely.

I could not pinch it tight enough to hold. Subsequent examination showed no sign upon the gold wire.



This proves conclusively that, under these circumstances, the pinch of a round wire is an insecure hold against torsion.*

I cannot but regret this result, for the sake of the exquisite workmanship; but there is no help for it, the wires must be held in some other way before anything can be done to test the instrument as a gravimeter. I find, for instance, that a slight flattening of the wire meets the slipping difficulty perfectly; but a flattened wire strains (*i.e.*, takes up a twisted or strained condition) very readily, and it is doubtful whether any alteration of the cylindrical form, even for a *very* small part of the length, can be permitted without risk.

My examination having reached this stage, I found it necessary to decide whether to stop here or run some risk of injuring the apparent perfection of the instrument, in the endeavour to obtain the first requisite, a firm hold of the ends of the torsion wire. I defer, for the present, all reflections in order to proceed with my narrative. I decided to flatten the wire slightly at the place where it passes the split. Whether I did so insufficiently or not at the right place it is impossible now to say. The result was slipping, as before. I then determined to have recourse to the loop—by which I mean giving to the wire at the place where it is pinched a sharp bend back upon itself. To do this the whole instrument had again to be dismembered. I cannot give an adequate idea of the anxiety attending a step of this kind, in the case of an unfamiliar instrument of delicate construction. My anxiety would have been greater perhaps, but hardly my care, had I known, what I now learnt, that the gold wires were nearly as brittle as untempered steel. In dismounting the major weight (without which the lower attachment of the single wire could not be reached) one of the double suspending wires snapped off short at the fastening. I did not recognise the cause until on attempting to double back the single wire to give it the crook it also snapped short. These mishaps were experienced without much cost, for the new plan demanded but a fifth of an inch of wire; which the other end, in each case, could easily spare. But before trying to bend it again I took the precaution to anneal the end. At length, after a deal of trouble, I had succeeded in fastening in this way both ends of the single wire, and of the broken one of the pair. I ought to have done the same with the other as well, but courage was wanting to go further in this direction than accident had rendered necessary. The torsion on so long and thin a wire (the pair are much thinner than the single one) turned through only one quarter of a revolution, would probably be so slight as not to exceed the holding power of the existing attachment.

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* I have since tried a steel wire pinched between lead sheets in a vice. No pinch seems sufficient to prevent the slipping, if the wire is straight, but a slight crook suffices.

I should say here, that before putting the parts together again, I weighed them, and measured the wire-lengths. The weighing is elsewhere recounted. The lengths are 12·6 and 3·08 inches respectively. I also took the opportunity to measure exactly the diameter of the glass plunger, having reason to doubt the correctness of the measurement assigned in Mr. Broun's description. The result justified my suspicion.

The end of all this is now at hand. In due time the instrument was once more in a condition to recommence the adjustments. Warned by previous experience I wasted no time over perfecting the first; but, noting the actual position of the lower mirror when at rest, I turned it through two revolutions and allowed it to return. *It failed to reach its former place.*

I tightened the holding nuts to the utmost which the metal would bear; with no better result than to reduce the slipping, but not to prevent it. The index test seemed to exonerate the upper holding, but there was either slipping or straining—and that to a variable extent—on every trial, whichever way the lower weight was turned.

At this point, I decided on abandoning the investigation, until I should receive further instructions. Considering that the instrument has been entrusted to me to experiment *with* rather than *upon*, I have already dared more perhaps than I ought to have—certainly more than most persons would have felt justified in doing. I presume, perhaps, in thinking that a rather long experience of instruments of precision will be accepted as my excuse for having gone so far; in the earnest endeavour to ascertain whether an instrument of such exceeding beauty (as to workmanship) would prove as valuable as it looked, and as, I must say, the principle of its construction leads one to expect. In pursuing this endeavour I have done some slight injury to it—which can be easily remedied if necessary. This I admit: but *per contra* I have ascertained a good deal without which it would be useless, besides gaining some experience which should avail in perfecting it, and making it (or another similar in principle) useful for its intended purpose. Finally, if any further apology is necessary, I will add this—that in no case could the instrument have been actually and *efficiently* employed for that purpose without material alterations.

I cannot too often repeat, that both as regards the design and its execution, the instrument deserves high praise. Nevertheless it is a failure. I have felt this all along, and I ought not to conclude this paper without pointing out what I conceive to be its chief defects. Of the defect which has brought this trial to a premature conclusion —the insufficient hold of the ends of the wires—it is only necessary to say that some plan should be discovered of putting this beyond question. This is the *first* consideration. I do not regard this as an error either of design or of construction such as can be complained of. But I do regard as such that which gives the instrument, as actually constructed, its beauty, viz., the minuteness and delicacy of its parts. There is a wealth of adjustment which is not necessary, and their details are all on far too small a scale. I suppose that the major weight consists of not less than 100 parts. Of these probably 80 could be set apart whose total weight would not reach 200 grs. out of the 3,100 which the whole weighs. It is clear that, supposing the total to be restricted to that, a more generous distribution to the smaller parts might have been made, without any disadvantage; but on the contrary, a great gain in handiness. I fear it is useless to add that a large proportion of these smaller parts are of the kind which instrument makers delight to show their wonderful skill in producing —as nature does flowers. Of course, one cannot tell how far these multifarious adjustments may not owe their presence to a conscientious endeavour on the part of the maker to give effect to his instructions: but that must not prevent my saying that they are, to a large extent, redundant, unnecessary, if not useless.

I regard it as a mistake that the whole instrument is on so small a scale. We should not be far wrong in estimating the power of such an instrument, not *in proportion* to its size, but in an even higher ratio. Even in scale-balances there is an advantage in size; though there it is the *absolute* weight put into the scale which is measured, whereas in the gravimeter we have a balance whose delicacy is measured by the *relative* minuteness of the weight added. The torsion gravimeter relies on the perfect obedience of the bifilar suspension, due to the absence of friction. Until it can be practically shown that this obedience is, in practice, *not* perfect, it is an abuse of the leading principle to refrain from drawing upon the resource it offers. This is what is done when high constructive art and skill are exerted to keep down the weight and size of parts instead of the contrary.

The minor weight is turned as one turns the hand of a clock with the finger. I cannot imagine why this one-sided action is preferred—for I suppose it is preferred—to the obvious two-fingered action, which comes into play in so many common practices where one wishes to avoid displacing the central axis of motion. The effect is to give a wobbling swing to the whole hanging system, increasing the risk of jar and strain, besides displacing the centre, and thereby altering the normal direction of the pull of the lower weight as well as the verticality of the mirrors. [It is true that magnets are provided to relieve this, but I hardly suppose any one desiring to make accurate observations would allow them to remain as preferable to the two-fingered stop.] I have already remarked on the so-called "finely-polished agate points," which I have been obliged to replace by steel ones, because being very thin and of glass they soon got broken off. I do not recognise any objection to their being of metal.

A tripod stand is furnished with the instrument, and I always used it; but it is very unsuitable, for the following reason. A portable tripod almost necessarily requires a large splay; this involves risk—even in the hands of a surveyor habituated to three-legged stands—and risk of a kind which, in my opinion, is fatal in the case of such an instrument as this. This is why I notice the stand. I doubt if anything like the necessary permanence of condition could be looked for in a fine wire which, when supporting such a weight as 600 grs., had to sustain a jolt, or such a shock as would be caused by a slight kick to one of the legs of the stand. I have no proof of this. It is one of the tests I intended to inflict on the instrument. In the absence of anything but a strong doubt, I can do no better than set it down here for future trial.

The last point which I shall dwell upon is one which has already been noticed, the effect of torsion of the double wires in *deferring* the position of unstable equilibrium beyond that of maximum gravity resistance. As already pointed out, the possibility of choosing 90° as the place of rest turns on the alliance of this torsion with the force of gravity in opposing the torsion of the single wire. If Mr. Broun was aware of this, it is strange that he gives no hint of it in mentioning the forces in equilibrium; anyhow, the third force is there, and might, I think, be taken advantage of. I found by trial that the torsion of the single wire might be increased so cautiously as to cause the major weight to stand nearly stationary at the position of unstable equilibrium. If the angle of position of the lower weight were read off (by vernier or otherwise) when this happened, both right hand and left hand, I imagine a delicate measure of the variation of gravity would be obtained, without the need of any appliances for varying the weight, or of a collimator. It would be foreign to the purpose of this paper to pursue this design further here; it is enough to have indicated it in connexion with the principle involved in Broun's design. I will only add that the two principles here indicated, viz., the balancing of gravity by torsion, and the determination of the condition of equilibrium when unstable, are both involved in a simple bifilar suspension; and I see reason to think that this form of gravimeter, from its extreme simplicity and great adaptability, is worth consideration.*

It now only remains to offer some apology for the length of this report, and for its discursiveness. It will have been quite apparent to any one who has had the patience to read it, that it has been written from day to day, as the examination proceeded; and now that the latter has to be closed without anything of the nature of an "observation" having been possible, it may be that there is a certain advantage in letting the facts, so brought forward, tell their own tale. Much, no doubt, could well be spared, but at the risk of laming the narrative. . . . . At the same time, I may say that I am quite willing to renew the attack, and do what can be done to reach a more promising conclusion, and to make the instrument efficient; provided I am so instructed. In that case, however, the sanction of the Royal Society, to whom it belongs, will be also necessary; as alterations may be necessary which one would otherwise have no right to make.

---

* If $(P+p)\frac{r}{R}\sin\theta=2\rho\phi$ be the general statical equation of a simple bifilar system, the equilibrium is unstable when $\sec\theta=-\frac{P+p}{2\rho}\cdot\frac{r}{R}$, in which position $\phi_0=-\tan\theta_0$. From this it is at once apparent, since ϕ must be positive, that θ_0 must lie between 90° and 180°. It must also be greater than 90°, or ϕ_0 would be infinite; but beyond this I do not see any theoretical restriction. I think it would be advisable to make ϕ_0 as large—several revolutions—as the wires will bear without injury. All that is necessary is that they be provided with means of turning their upper ends, until the weight reaches its position of unstable equilibrium (which should happen when θ is little more than 90°), and with means of recording the angle through which they have been turned so as to reach this condition. The applicability of such an arrangement, as a sensitive gravimeter, will depend entirely on the adaptation; which, I believe, would be found quite feasible. The equations here given are sufficient to determine suitable proportions.

I append a separate account of the weighings of the major and minor weights. Supposing the instrument ultimately brought into use, it would become necessary to know the total weight of the suspended parts with some accuracy. This need should be kept in view, if alterations are made.

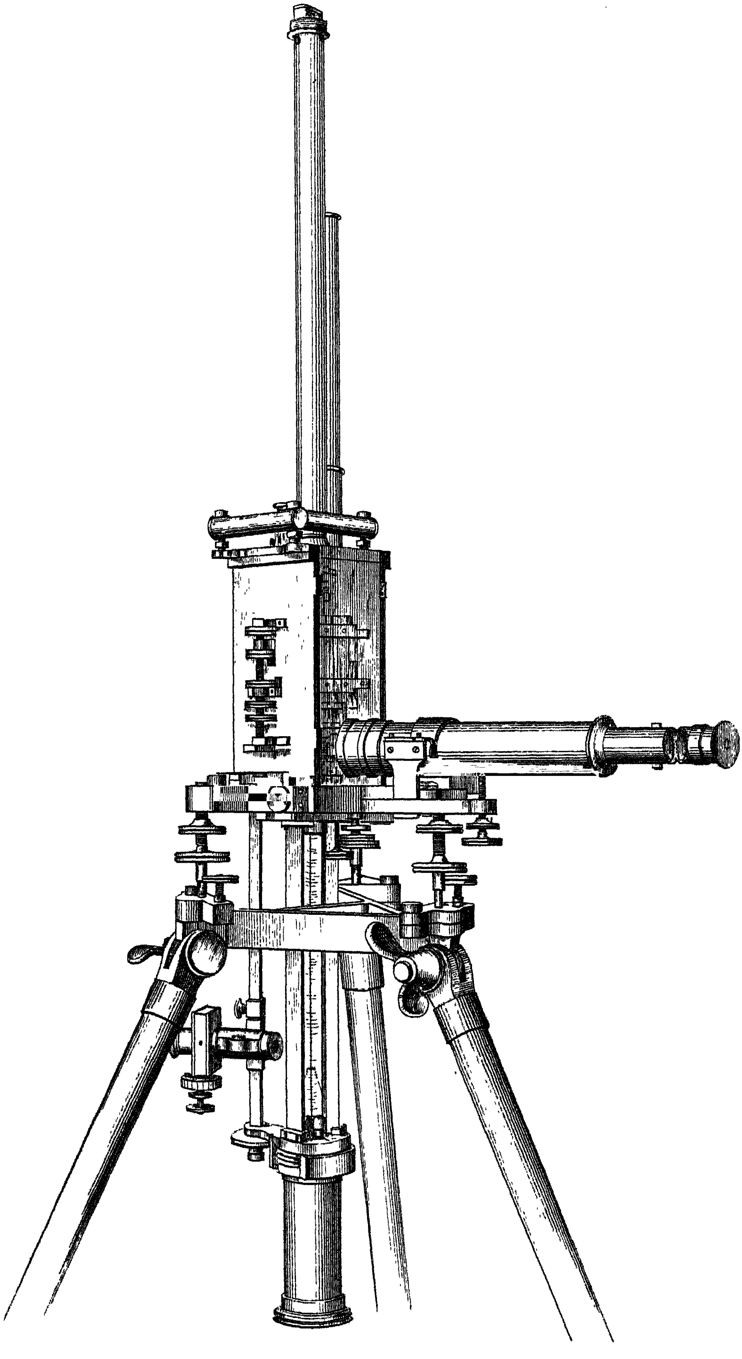
Account of the Weighing.

I have weighed the suspended parts on three occasions, the circumstances differing in each case somewhat. In the first two weighings, the cross piece at the top of the shaft having been removed, the whole of the swinging parts were suspended from one arm of a balance—with exception of three mirrors absent for repair at the time. This suspension involved certain additions and subtractions by way of allowance for parts not present or redundant. These were either determined or estimated for. I have since been able to correct all the estimations, and the first weighing makes the sum of the two parts A and B equal to $3,721+69+14-35-18-22 =3,729$ grs.

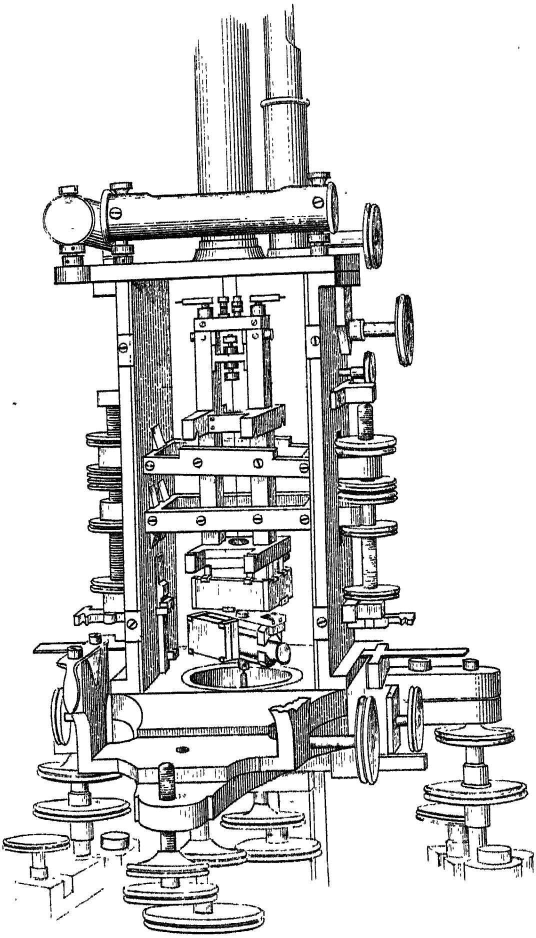
In the second weighing—being uncertain of the accuracy of the weights used—I prepared two lead blocks representing approximately the masses A and B; and, by more directly counterpoising the redundant parts, and reducing to a minimum all corrections, I obtained a mass A equal to 3,109 grs. and a mass B equal to 615 grs., the sum of which or 3,724 grs. balanced, or would have balanced (for I could not eliminate two small pieces which had to be allowed for) the whole of the suspended parts. The separation into two parts provided for the removal of the lesser when the minor weight was supported. I was quite aware that this was a doubtful partition, owing to the impossibility of *exactly supporting* the lower weight without imparting a thrust through the single wire. But there was no way of obviating this without a separation of the two parts of the mechanism. The *total* weight was free from suspicion.

When later events led me to take the whole apparatus to pieces, I took care before building it up again to weigh the two principal parts separately—as well as to take some measurements which seemed important. The result showed that this thrust had been much stronger than I supposed, having transferred 9 or 10 grs. from the lower to the upper estimate. I now found, directly and without any allowances or reductions—the large mass being in one scale, and the major weight *as it would hang* in the other, that the former required paring down. The parings weighed 9 grs. Conversely the smaller mass required an addition of 10 grs. to balance the minor weight. Having made this transfer, I now have the two leaden masses, repre-

Herschel. *Proc. Roy. Soc. Vol. 32. Pl. 8.*



W. H. Wesley del. W. Rlein sc. West Newman & C° lith.



[illegible]		West Newman & C° [illegible]

senting (each within 1 gr., I think) the major and minor weights, as they actually hang: they weigh (according to my scale weights) 3,100 and 624 grs. respectively. The sum agrees with the second weighing to a grain—the exact agreement being unintentional.

These weighings do not include the platinum wire riders.

I may add here an account of the measurement of the glass plunger—described by Mr. Broun as "calibrated" to 1 millim. My rough (?) measurement had indicated, as above said, ·048 inch as its diameter. The cathetometer microscope seemed to show it about 120 divisions, or 1·2 millims.$=\cdot049$ inch, but I did not trust this. I hung a small weight by a fibre of raw silk and wound up 100 turns of it on the glass rod. The length absorbed was 14·9 inches. I estimate the diameter of the silk, which was coarse, at ·001; but it is very uneven. This would give $\frac{0\cdot149}{\pi}-\cdot001=\cdot046$ inch. Not satisfied, I repeated this with some fine silver wire, annealing it first. This gave similarly $\frac{0\cdot154}{\pi}-\cdot0036=\cdot0454$ inch. The diameter of the wire was got by measuring the length covered by the close coil of 100 turns. The true diameter of the glass rod is rather larger than this last, as the outer part of the wire would stretch more than the inner would compress. It may be taken as ·046 inch at the place chosen, which was unfortunately near the top. As 1 millim. is ·0394 inch, Mr. Broun's statement on this point must be rejected.

I cannot imagine on what ground this plunger has been made of glass. Surely it cannot be contended that a metal wire would be of uneven diameter, in a way that a glass one would not be? On the other hand the risk of injury is considerable, and such a rod would be impossible to replace in foreign parts.

DESCRIPTION OF PLATES 8 AND 9.

As has been mentioned in the note to p. 519 the plates were prepared subsequent to the submission of the Report, which they are intended to illustrate.

Plate 8 is a general oblique view of the gravimeter on its tripod stand. The chief parts seen are—the *shaft* through which the parallel wires descend:—the *chamber* through the glass front of which is seen obscurely what is represented very faithfully in Plate 9:—the collimator, or observing telescope, to which a somewhat undue prominence is given in the drawing, by the effect of foreshortening:—the *table* and foot-screws:—the *tripod* stand:—the *well* and glycerine reservoir, with the cathetometer on the left.

There is hardly any part of this plate which requires more special explanation than will be found in the foregoing pages, if we except perhaps the arrangement of nuts and screws on the outside of the chamber; and these will be readily understood by consulting the other plate, where it is seen that they serve to govern the positions of the two stages inside by which the hanging part (the major weight of the Report) is held so as to maintain a fixed position, with slackened supporting wires, when packed. There is a different arrangement for holding the minor weight, which the engraver has failed in representing so well as the rest.

Plate 9 shows also a variety of milled screw-heads, each of which has of course a purpose; but as neither of them, except one by which the lower weight is turned round, has any part in the observations such as they are described above, but only in ultimate manipulations which it is unnecessary to dwell upon here, they may be regarded as ornaments. It will be noticed that the collimator is removed, and one of the side supports is supposed broken off, to discover the minor weight; which last, with its mirror and magnet bar, is seen turned through 30° or 40° into an oblique position. At the same level and outside the chamber are seen arms, one of which resembles a cross. These are the guides for two magnets which have been removed. Their intended purpose is mentioned by Broun in his description.

The pillar alongside the shaft is a case for a thermometer, the bulb of which is within the chamber.

"On the Coefficients of Expansion of the Di-iodide of Lead, PbI_2, and of an Alloy of Iodide of Lead with Iodide of Silver, $PbI_2.AgI$." By G. F. RODWELL, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Professor A. W. WILLIAMSON, For. Sec. R.S. Received March 10. Read March 31, 1881.

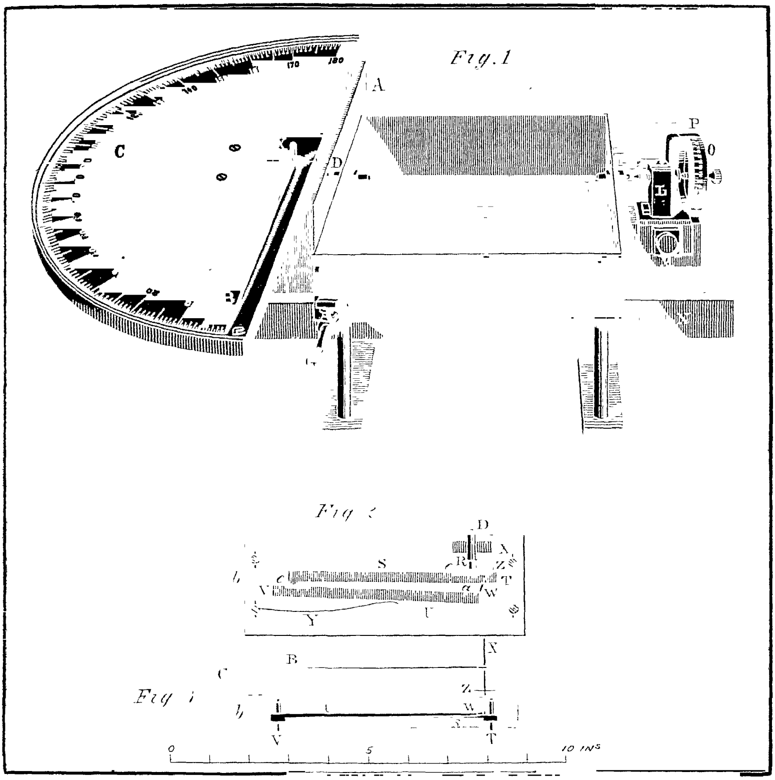
In former communications which I have had the honour of submitting to the Royal Society, I have given determinations of the coefficients of expansion by heat of the chloride and bromide of silver and the iodide of mercury between 0° C. and the fusing point; also determinations of the coefficients of expansion and contraction of the iodide of silver, and of certain chlorobromiodides of silver. ("Proc. Roy. Soc.," vol. 25, pp. 280–303, and vol. 28, p. 284.)

The iodide of lead, and an alloy of iodide of lead with iodide of silver, were thought to be very suitable substances for a continuation of these experiments. The following pages describe the results obtained.

The experimental method was precisely similar to that before described, but the expansion apparatus was rendered more delicate by several notable changes suggested during the course of the former experiments. It is unnecessary to describe this apparatus again (for description *vide* "Proc. Roy. Soc.," vol. 25, p. 281–2), but it may be remembered that a homogeneous rod of the substance under examination is connected with a series of levers which multiply 5,382 times, while the value of the movements is estimated by a micrometer screw reading to $\frac{1}{50000}$ of an inch. The following alterations were made mainly with a view of reducing the resistance by diminishing friction, and thus adding to the sensibility of the apparatus :—

1. The wooden base N (fig. 1) was replaced by a massive stone

block, to which the box containing the levers and the upright carrying the micrometer head were firmly bolted.



This block with figs. 1, 2, 3 is a reprint from "Proceedings," vol. 25, p. 280-282, where all are described.

2. The levers S, U (fig. 2) were reduced in weight.

3. The spring Y (fig. 2) was removed, as it was found that the recoil of the helical spring Z was quite sufficient to bring the index back to zero, and the presence of Y served only to increase the resistance and general strain.

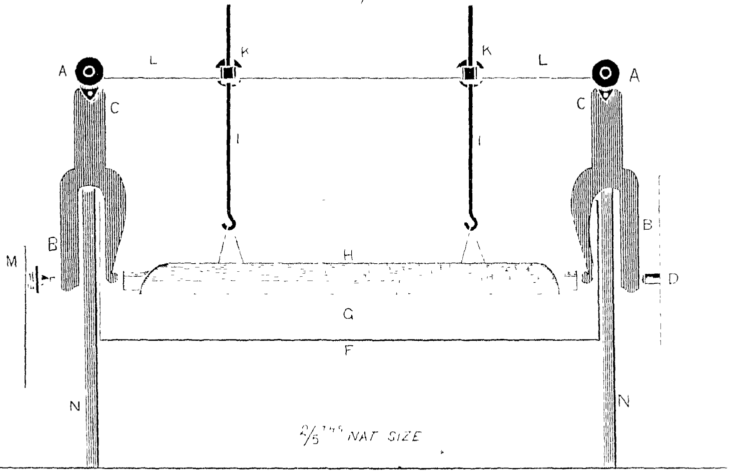
4. The vertical axis X (figs. 1 and 3) was no longer allowed to work in the upper confining plate of the framework *b*, but it was caused to turn lightly on a bent arm above, while below it rested upon a slightly hollowed ruby.

5. The steel chain W communicating motion from the lever U to the vertical axis X, which carries the index, was shortened and caused to wind upon the barrel in such a manner that when unwound to the

extent of half a single coil, it moved the index through its entire range, viz., from 0° to 180° of arc. By this means any possibility of the chain doubling upon itself was obviated.

6. But perhaps the most important changes were the removal of stuffing boxes I and the rods H from the trough F, and the substitution of levers working over the rim of the trough; and the suspension of the rod of substance under examination in a cradle between the levers. This was effected in the following manner:—

Fig. 4.

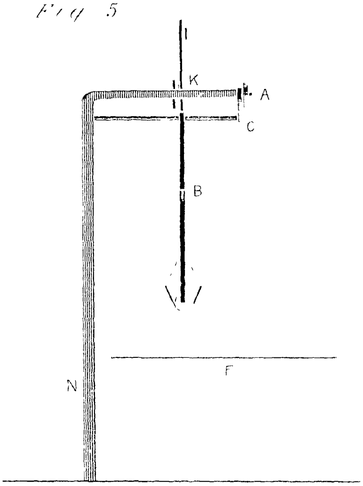


Section through the trough longitudinally, showing the mode of suspension of the bar, and the position of the levers.

A horizontal bar LL (fig. 4) was supported by rods N, N, strengthened by cross bars (not shown) let into the stone base of the instrument; it carried Y-shaped brass levers B, B, moving about axes at C, C (figs. 4 and 5), attached at the points A, A. F is the trough in which hot ceresine is used for heating the bar under examination, H, supported by the cradle G. Two rods I, I, which slide in holes K, K, and are capable of being held at any height by screws, support the cradle G. D is the rod (figs. 1 and 2) which bears upon the lever S, and M the point of the micrometer screw.

The apparatus was standardised at frequent intervals by the use of a rod of fine homogeneous silver. Ceresine boiling at 430° C. was used to heat the rods in F, and it was heated to any desired temperature by means of a Bunsen burner placed beneath, and near the centre of the trough.

Fig. 5



End section of the trough, showing one of the levers, and the bar which carries it.

Iodide of Lead.—Pure iodide of lead was cast into rods one-third of an inch in diameter and 6 inches long. The ends were made plane by a fine steel saw, and they were furnished with copper caps. Great difficulty was experienced in casting the rods, owing to the brittleness of the iodide. Slightly greased tubes of very thin German glass were used as the moulds, and as the rods would rarely slip out of the tubes the glass had usually to be chipped away along the whole length of the rod by the point of a knife. The iodide underwent the same changes of colour as were observed in the iodide of silver; that is to say, it fused to a bromine-red liquid, which, when solidified, became red-brown, and, while cooling, brick-red, reddish-yellow, and, when completely cool, orange-yellow. Harsh noises, like those produced by bending tin, were heard during the cooling of the mass, and the fracture was highly crystalline.

Differences of opinion appear to exist as to the effect of fusing iodide of lead in the air. In the same volume of a standard work I find two exactly contrary opinions: for it is stated, on the one hand, that the iodide if fused in contact with air gives off a part of its iodine, becoming oxyiodide of lead; while elsewhere the iodide is classed among those which may be fused in an open vessel without change.

In order to set this matter at rest, 56·1690 grms. of iodide were fused in a covered porcelain crucible. The fusion was continued for eight minutes, during which the cover was three times momentarily removed. Violet fumes of iodine escaped on each occasion, but on weighing the loss was found to amount to only

·11036 per cent.

Again, the mass was kept fused for four minutes, and the crucible cover was twice removed, but the loss had only increased to

·1584 per cent.

After a third fusion the total loss only amounted to

·1718 per cent.

Hence it is manifest that iodide of lead may safely be fused out of contact with air, with scarcely appreciable loss. When, however, the crucible cover was permanently removed, the iodide rapidly decomposed.

When the iodide was heated in a current of carbonic anhydride, it sublimed unchanged in crystals; while if it was heated in a current of dry oxygen it rapidly decomposed, fine crystals of iodine collecting in the fore part of the tube.

The specific gravity of iodide of lead, in common with the iodides of copper, silver, and potassium, is less than the mean specific gravity of its constituents. Karsten found it to be 6·0282, Boullay 6·11, and my own determinations gave 6·12. The calculated specific gravity is 6·629.

The fusing point as determined by Mr. Carnelley is 383° C.

The coefficient of cubical expansion for 1° C. was found to be

·00007614

for temperatures between 0° C. and 205° C. It increased to

·00008317

between 205° C. and 253° C.

Between 253° C. and 265° C. the mass expanded rapidly, with a coefficient nearly eight times greater than the previous, viz.:—

·0006378.

After the subsidence of this rapid expansion it no longer retained the original coefficient, but assumed one of more than double the amount, viz.:—

·000180.

At temperatures some distance from the melting point the rod began to bend, and it became necessary to assume that this last coefficient continues to the melting point. The expansion in passing from the solid to the liquid condition was determined by the method described in my previous paper.

It will be observed that the iodide of lead, as in the case of the iodide of mercury ("Proc. Roy. Soc.," vol. 28, p. 284), has three coefficients of expansion, viz.:—(*a*) a coefficient somewhat less than that of chloride of silver up to 253° C.; (*b*) a coefficient during 12° C., nearly eight times greater than the preceding; and (*c*) finally a coefficient somewhat more than twice as great as that between 0° and 253° C., at temperatures above 265° C. Undoubtedly the iodide of lead, as in the case of the iodide of mercury, undergoes a molecular change while rapidly expanding between 253° and 265° C., and before assuming the higher coefficient. This is supported by the fact that the highly brittle and crystalline rod showed itself capable of bending after having undergone the rapid expansion. It will be remembered that the iodide of silver, which is very crystalline and brittle below 145° C., becomes amorphous and plastic above that temperature. The familiar example of sulphur will also recur to the mind.

If we suppose a mass of iodide of lead to be heated from 0° C. to the melting point (383° C.) the following will be the volumes at the respective temperatures.

Volume at	0° C.	=1·000000.
,,	205°	=1·015608.
,,	253°	=1·019595.
,,	265°	=1·027248.
,,	383° (solid)	=1·048488.
,,	383° (liquid)	=1·078080.

The curve is shown in Table A.

The specific gravity of the iodide in the molten condition is 5·6247.

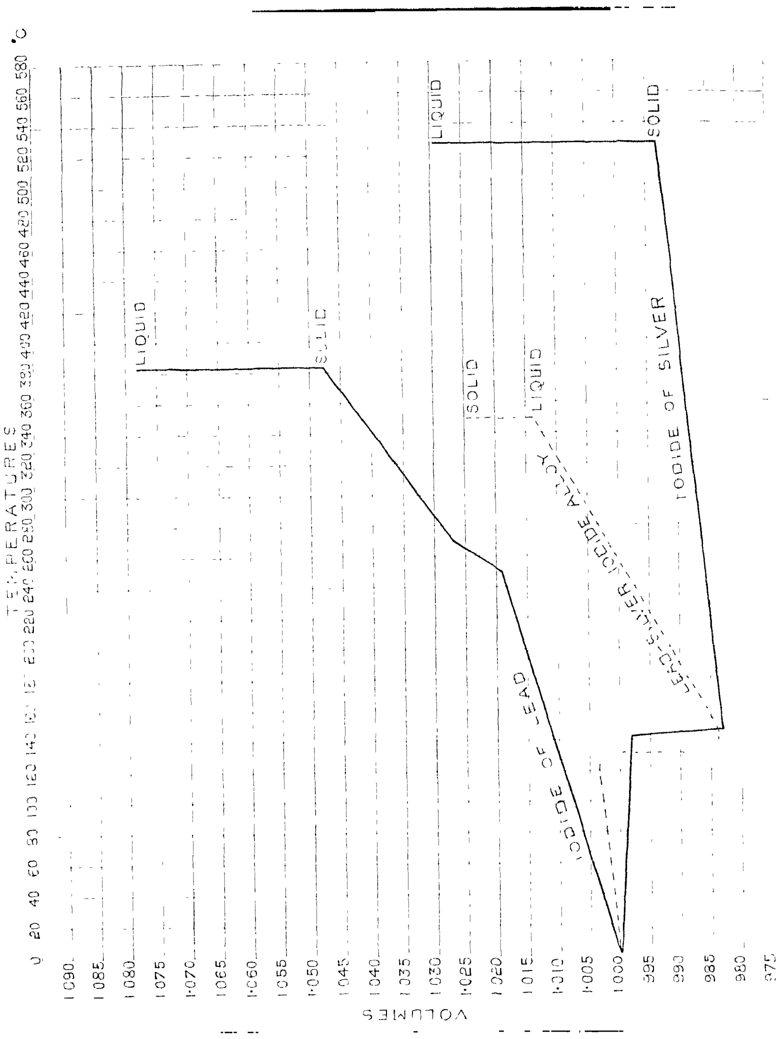
The fact that a substance may possess two or three different coefficients of expansion has apparently only been observed hitherto in the case of such substances as fusible alloy, because in determining the coefficients of solid bodies temperatures exceeding 100° C. have rarely been employed. Paraffine or ceresine used as a heating medium will allow the determination of coefficients to a temperature of 300° C., and, undoubtedly, many bodies when thus examined would be found to present anomalies similar to those remarked in the case of the iodides of lead and mercury.

The Lead-Silver Iodide Alloy.

Bearing in mind the peculiar nature of the coefficients of certain alloys of iodide of silver with the chloride and bromide (" Proc. Roy. Soc." vol. 25, p. 292), it was thought to be advisable to determine the coefficients of an alloy of iodide of lead with iodide of silver.

These bodies were accordingly fused together in the proportion of one molecule of each, viz., $PbI_2.AgI$. This contains in 100 parts

Table A.—Table showing the Relationship between the Temperature and Volume of Iodide of Lead, Iodide of Silver, and of the Lead-Silver Iodide Alloy, $PbI_2.AgI$.



Iodide of lead......	66·206	Lead............	29·7449
Iodide of silver	33·794	Silver.	15·5642
		Iodine...........	54·6909
	100·000		100·0000

The substances were fused together in a porcelain crucible, and cast in thin glass tubes 9 inches long by one-third of an inch in diameter. The molten mass underwent the same changes of colour in cooling as either one of its constituents, and ultimately became a dull orange-coloured compact mass. Although composed of two substances which are highly crystalline and brittle, the alloy was found to be hard and tenacious. Although the constituents are coarsely crystalline in structure, the alloy is finely granular. During the cooling of the mass it expanded with sufficient force to break the glass tube. Harsh noises were emitted during cooling, and the whole mass was sometimes jerked from its position; while, if held in the hand, it was felt to be agitated by strong tremors.

Mr. T. Carnelley has determined for me the melting point of the alloy, which he finds to be 350° C.

The specific gravity is 5·923.

By repeated digestion with large volumes of boiling water the alloy is decomposed, the iodide of lead being dissolved, while the iodide of silver remains as a dull green powder.

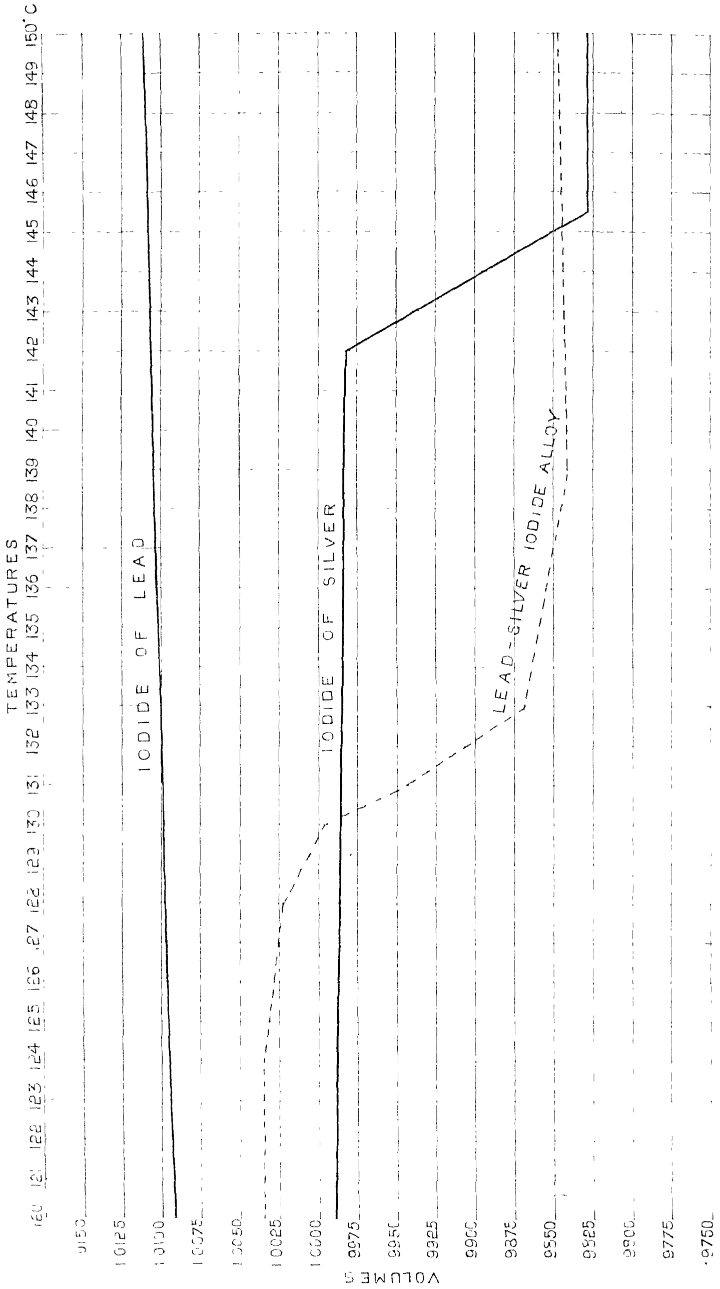
On examination in the expansion apparatus the alloy was found to undergo slow expansion to a temperature of 118° C., then, for 6° C., it simply absorbed heat without either contracting or expanding. At 124° C. contraction commenced, and continued at unequal rates till a temperature of 139° C. was attained. Then, again, the mass underwent neither contraction nor expansion during heating through 5° C., and then it commenced to expand somewhat rapidly. The most rapid contraction on heating took place between 130° and 133° C. Thus, in all, for the temperatures during which the mass contracted, the index moved through fifteen revolutions of 180° to 0° of arc, and these were related to the temperatures in the following manner:—

1 revolution of index took place during heating from 124—128° C.
2 ,, ,, ,, 128—130° C.
4 ,, ,, ,, 130—131° C.
6 ,, ,, ,, 131—133° C.
2 ,, ,, ,, 133—139° C.

The details of these contractions are shown in Table B.

The heating, especially at these temperatures, was excessively slow, and so moderated that a complete observation of the behaviour of the substance in the expansion apparatus lasted from three to four hours. Above 144° C. the alloy expanded with a coefficient about three times greater than that which it possessed between 0° and 118° C.

Table B.—Details of the Contraction by Heat of Iodide of Silver, and of the Lead-Silver Iodide Alloy.



Coefficients of Cubical Expansion and Contraction of the Alloy for 1° *C.*

Between	0°	and	118° C.=	+·0000306
„	124	„	128° C.=	−·0003240
„	128	„	130° C.=	−·0012990
„	130	„	131° C.=	−·0017330
„	131	„	133° C.=	−·0039000
„	133	„	139° C.=	−·0004329
„	144	„	350° C.=	+·0001150

Plus has been placed before the coefficients of expansion on heating; *minus* before the coefficients of contraction. The expansion in passing from the solid to the liquid condition was determined as before.

The coefficient between 144° C. and the fusing point increased rapidly with the temperature.

If we take the volume at 0° C. as unity, we have the following volumes corresponding to the temperatures given:—

Volume at	0° C.	=1·000000
„	118	1·003610
„	124	1·003610
„	128	1·002314
„	130	·999716
„	131	·994517
„	133	·986717
„	139	·984120
„	144	·984120
„	150	·984810
„	300	1·006500
„	350 (solid)	1·013790
„	350 (liquid)	1·024370

In regard to this alloy the following points may be noted:—

1. It possesses a similar density at three different temperatures. Thus, it is obvious that the density is the same at 0° C., at just below 130° C., and at 282° C.

2. Although the alloy contains only 33·794 per cent. of iodide of silver, it contracts as considerably *during heating* as the iodide itself.

3. While the iodide of silver commences its contraction at 142° C., and finishes it at 145°·5, the alloy commences to contract 18° C. lower (viz., at 124° C.) and finishes 6°·5 C. lower (viz., at 139° F.).

4. The chlorobromiodides of silver also began to contract on heating (an effect which, of course, we must attribute solely to the presence of iodide of silver) at 124° C., but they finished at 133° C.

5. The harsh sounds emitted by the alloy during cooling, and the tremors simultaneously propagated through the mass, prove that violent molecular agitation is going on at such time as the iodide of

silver is passing from the amorphous plastic condition to the brittle crystalline condition, within the mass of the iodide of lead.

6. The fusing point of the alloy is 177° C. lower than that of the iodide of silver, which constitutes one-third of its weight, and 33° C. lower than that of the iodide of lead, which constitutes two-thirds of its weight.

7. If the lowering of the fusing point (also markedly apparent in the case of the chlorobromiodides of silver) is due to the fact that similar particles of matter attract each other more powerfully than dissimilar, and hence, when the particles of two bodies are mutually diffused, the attraction becomes less, and the molecular motion is consequently more readily assimilated; the same cause may serve to explain the commencement of the phase of contraction on heating the alloy at a temperature 18° C. lower than the substance to which it owes this property.

8. It is interesting to compare one of the chlorobromiodides of silver with the lead-silver iodide alloy. For this purpose we will take the chlorobromiodide which contains the nearest approach to the same quantity of iodide of silver as the alloy. The second of the chlorobromiodides before described ("Proc. Roy. Soc.," vol. 25, p. 295) contains 41·484 per cent. of iodide of silver, and 58·5160 per cent. of the chloride and bromide of silver, which latter, from the heat point of view, may be regarded as the same substance, because their coefficients of expansion are practically the same. It may be noted (*vide* below) that while the expansion of the bromide (which is slightly greater than that of the chloride) scarcely exceeds that of the iodide of lead, and while, moreover, the chlorobromiodide contains 8 per cent. more iodide of silver than the lead-silver iodide alloy, the amount of *contraction* by heat of the latter is *more than twenty times greater* than that of the former, although we must believe this effect to be solely due to the iodide of silver in each case.

Comparison of the Coefficients of the Iodide of Lead and the Bromide of Silver, used in conjunction with Iodide of Silver in the formation of the two Alloys given below.

	Iodide of lead.	Bromide of silver.
Melting point	383° C.	427° C.
Volume at 0° C.	1·000000	1·000000
„ 205	1·015608	1·021945
„ 253	1·019594	1·027369
„ 265	1·027248	1·028725
„ 383 (solid)	1·048488	1·042531
„ 383 (liquid)	1·078080	
„ 427 (solid)		1·047855
„ 427 (liquid)		1·107225

Comparison of the Coefficients of the Lead-silver Iodide Alloy with those of a Chlorobromiodide of Silver, and of Iodide of Silver.

	Lead-silver iodide alloy, containing 33 ·794 per cent. of iodide of silver.	Chlorobromiodide of silver, containing 41 ·484 per cent. of iodide of silver.	Iodide of silver.
Composition	PbI_2AgI	AgI,AgBr,AgCl.	AgI.
Volume at 0° C.	1 ·000000	1 ·000000	1 ·000000
„ 118	1 ·0036108		
„ 124	1 ·0036108	1 ·012037	·998765
„ 128	1 ·002314		
„ 130	·999716		
„ 131	·994517		
„ 133	·986717	1 ·006637	·998608
„ 139	·984120	.. ..	·998503
„ 142	.. ..	.. ..	·998450
„ 144	·984120		
„ 144 ·5	.. ..	.. ..	·982700
„ 150	·984810	.. ..	·982828
„ 300	1 ·002060	1 ·039239	·987094
Volume of solid at melting point	1 ·013790	1 ·046646	·993550
„ liquid at solidification point	1 ·024370	1 ·097486	1 ·029450
Melting point	350° C.	331° C.	527° C.

The accompanying tables show the curves of contraction and expansion of the lead-silver iodide alloy, and of its constituents. In Table B the scale has been enlarged in order to show the details of the contraction of the alloy on being heated from 124° C. to 139° C.

(Addendum. Received April 8, 1881.)

Other alloys of iodide of lead with iodide of silver have since been made, having the following composition:—

(1.)	$2AgI.PbI_2$,	containing	50·517	per cent.	of iodide of silver.
(2.)	$3AgI.2PbI_2$	,,	43·360	,,	,,
(3.)	$4AgI.3PbI_2$	,,	40·497	,,	,,
(4.)	$5AgI.4PbI_2$	,,	38·950	,,	,,
(5.)	$10AgI.9PbI_2$	,,	36·190	,,	,,

These all possessed the same general appearance as the alloy $AgI.PbI_2$ described above, which contains 33·794 per cent. of iodide of silver. But with the exception of No. 5 they were all so brittle that they could not be cast into rods suitable for use with the expansion apparatus. During cooling large rifts appeared in the rod at right angles to its length, at the time when the iodide of silver commenced to expand. In the case of No. 1 the rod was violently broken during its cooling by the expanding iodide of silver; even when slowly annealed in hot paraffine. It may be noted that no such effect was produced in the case of the chlorobromiodides of silver, having the composition respectively: $Ag_2I_2.AgBr.AgCl$; $Ag_3I_3.AgBr.AgCl$; $Ag_4I_4.AgBr.AgCl$; and containing in each case a larger percentage of iodide of silver (viz., 58·6404; 68·0171; and 73·9285) than the silver-lead iodide alloy No. 1. The chlorobromiodides, although, of course, their brittleness increased with the percentage of iodide of silver, formed less brittle rods than the iodide of silver, and than the first of the silver-lead iodide alloys, although the latter contains 23 per cent. less iodide of silver than the chlorobromiodide $Ag_4I_4.AgBr.AgCl$.

OBITUARY NOTICES OF FELLOWS DECEASED.

Michel Chasles was born at Epernon, in the department of Eure-et-Loire, on the 15th of November, 1793.

He began his education at the Lycée Impériale, and even in childhood showed a decided taste for geometry, and was in the habit of communicating to the pupils of other schools the problems given him by his teachers, obtaining their problems in exchange.

In the year 1812 he began his studies at the École Polytechnique. In 1814 he assisted with his fellow-students in the defence of Paris, and in that same year passed the engineering examination. After considerable hesitation he decided to accept the appointment as officer of engineers to which he was now entitled; but just as he was about to enter the service he was induced to surrender the appointment in favour of a friend, who was next on the list to the successful candidates, and to whom it was a great object. He spent a short time with his mother at Chartres, and then resumed his studies at the École Polytechnique.

When, in 1814, all the students of the École Polytechnique were abruptly dismissed, Chasles gave hospitality in his home at Chartres to his brilliant schoolfellow at the Lycée Impériale, Gaëtan Giordini, who through his influence had been induced to study geometry, and who had obtained the first prize at the Concours Général over the head of Chasles himself, and had afterwards obtained the first place in the École Polytechnique.

On finally quitting the École Polytechnique Chasles spent about ten years in retirement at Chartres, devoting himself to geometry.

In 1837 he published the first edition of his great work, "Aperçu Historique sur l'Origine et le Developpement des Methodes en Géométrie," which was characterised by De Morgan as a work of great importance in the historical point of view, and described in the following words by M. Bertrand: "L'admirable 'Aperçu Historique,' qui sous ce titre plus que modeste, restera l'œuvre la plus savante, la plus profonde, et la plus originale qu'ait jamais inspirée l'histoire de la science."

In 1841 Chasles became Professeur de Machines et de Géodésie at the École Polytechnique, which appointment he held for ten years, when he resigned it in consequence of some radical alterations which were being made, and of which he entirely disapproved.

In 1846 he was appointed to a new chair of Modern Geometry at the Faculté des Sciences, which had been established in consequence of the strong recommendation of Poinsot, and for twenty-five years Chasles devoted himself assiduously to his duties at the Sorbonne. The "Traité de Géométrie Supérieure," an elaborate and masterly treatise which embodied the substance of a course of lectures given to the Faculté des Sciences, appeared in 1852. This book became scarce, but about two months before his death, Chasles had the satisfaction of seeing a second edition of it published, accompanied by his excellent "Discours d'Inauguration."

The "Traité de Géométrie Supérieure" was followed in 1865 by the first volume of his "Traité des Sections Coniques," being a sequel to the former. No other volume of this work ever appeared, though much desired by mathematicians.

In 1863 Chasles published his book "Les Trois Livres de Porismes d'Euclide, rétablis pour la première fois, d'après la Notice et les Lemmes de Pappus, et conformément au sentiment de R. Simson, sur la Forme des Énoncés de ces Propositions." The publication of this work led to a short controversy with M. P. Breton ("Question des Porismes—notices sur les débats de priorité auxquels a donné lieu l'ouvrage de M. Chasles sur les porismes d'Euclide," Paris, 1865; and a second part, Paris, 1866). M. Chasles comments on these in his "Rapport."

In 1851 Chasles was elected a member of the French Academy, of which he had been a corresponding member since 1839. In 1854 he became a foreign member of the Royal Society, and in 1865 he received the Copley medal, which was given to him in acknowledgment of his historical and original Researches in Pure Geometry. He was the first, and for some years the only, foreign member of the London Mathematical Society. He was also a member of the Cambridge Philosophical Society.

Chasles continued his labours in the cause of science without interruption, from the time of his leaving the Lycée Impériale until he was eighty years of age. An interval of sixty-eight years separates the first note of the pupil Chasles, which appeared in the "Correspondance sur l'École Polytechnique," from the last memoir he presented to the Academy. In the "Catalogue of Scientific Papers" will be found the titles of 177 of his papers, and it is computed that the number published since 1873 would probably bring the total to nearly 270. The subjects range over curves and surfaces of the second and of any degree, geometry, mechanics (and attractions), history, and astronomy. His "Rapport" perhaps furnishes the best key to his writings, and at pages 72—126, 220—280 will be found an account of his own contributions to geometry.

Chasles' life was a happy and simple one. He lived quietly and abstemiously, respected and loved by all the scientific friends whom he was in the habit of inviting and entertaining with such true kindness and hospitality. His brethren of the Institut, as well as the numerous other French and foreign savants, whom he liked so much to collect around him, will not easily forget the cordial and sympathetic reception which they always met with in his hospitable salon, whether in Paris or in his country quarters at Sèvres.

Chasles was an active member of the Council of the Société des Amis de France, and it is well known how diligently and conscientiously he always laboured in finding worthy objects for the charity of this institution, and how generously he frequently supplemented its work by donations from his own purse when the funds of the society did not suffice for its wants.

Fifteen years before his death, when the pupils of the École Polytechnique first conceived the idea of founding the Société Amicale, they perceived how important it was, in order to succeed in such an undertaking, that they should place it under the guidance of a man beloved and respected by all, and Chasles was requested to act as president. In spite of his already advanced age, and the necessity he might have urged of repose after his long labours, he accepted the post, and it is well known how zealously he devoted himself to the interests of the society, of which he must always be considered the real founder.

His death took place on the 18th of December, 1880.

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網走地方の黒色火山性土における連・輪作畑の土壌微生物特性と連作障害の要因解明およびその対策に関する研究

成　田　保三郎

昭和59年3月31日

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